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SINGAPORE

THREE ESSAYS ON CORPORATE FINANCE

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NANYANG BUSINESS SCHOOL

2021

THREE ESSAYS ON CORPORATE FINANCE

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A thesis submitted to the Nanyang Technological
University in partial fulfilment of the requirement for
the degree of Doctor of Philosophy

2021

Statement of Originality

I hereby certify that the work embodied in this thesis is the result of original research, is free of plagiarized materials, and has not been submitted for a higher degree to any other University or Institution.

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Authorship Attribution Statement

This thesis **does not** contain any materials from papers published in peer-reviewed journals in which I am listed as an author.

Chapter I is titled *How do individual politicians affect privatization? Evidence from China*.

This chapter is coauthored with Prof. Hong Ru. The contributions of the co-authors are as follows:

- Prof. Hong Ru initiated the idea, wrote the initial proposal, and designed the empirical analysis;
- I hand collected and analyzed the data;
- Prof. Ru wrote and led me to write several parts of the paper.

Chapter II is titled *Debt contracting and corporate social responsibility: The role of bank loan covenants*. This chapter is coauthored with Prof. Xin Chang, Prof. Tao Chen, and Prof. Endong Yang. The contributions of the co-authors are as follows:

- Prof. Xin Chang, Prof. Tao Chen, and Prof. Endong Yang initiated the idea and designed the baseline analysis;
- Prof. Endong Yang and I analyzed the data and wrote the empirical section and discussion section;

- All authors edited the manuscripts.

Chapter III is titled *Combating the COVID-19 pandemic: The role of the SA.RS imprint*.

This chapter is coauthored with Prof. Hong Ru, and Prof. Endong Yang. The contributions of the co-authors are as follows:

- Prof. Hong Ru initiated the idea and streamlined the empirical analysis.
- Prof. Endong Yang and I collected and analyzed the data.
- All authors wrote and edited the manuscripts.

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SUMMARY

How Do Individual Politicians Affect Privatization? Evidence from China

Abstract

This essay investigates how politicians' patronage connections affect privatizations in China. The connections to top political leaders (i.e., Central Committee of the Communist Party of China) make local politicians engage more in rent-seeking by selling state-owned enterprises (SOEs) at substantial discounts. These connected local politicians are also more protected in anti-corruption investigations, thus extracting more rents by selling SOE assets at substantial discounts. Consequently, the privatizations conducted by the local politicians with patronage connections achieve significantly lower gains in efficiency and performance. To identify the role of patronage connection in privatization, we use the mandatory retirement age cut-offs of Central Committee members in the regression discontinuity design. We find drops in price discounts of privatization deals and jumps in efficiency for privatized SOEs when local politicians lose connections to Central Committee members around the retirement age cut-offs.

Debt Contracting and Corporate Social Responsibility: The Role of Bank Loan Covenants

Abstract

This essay studies the real and social impacts of debt contracting by exploring the effect of loan covenants on borrowing firms' CSR (corporate social responsibility) performance. We document that firms with higher covenant intensity (i.e., more restrictive covenants) have poorer CSR performance. The endogeneity tests, which include an instrumental variable approach based on supply-side determinants of loan covenants and two quasi-natural experiments respectively based on SFAS 160 and anti-recharacterization statutes, support a causal relation running from covenant intensity to CSR. Further analyses reveal that the negative CSR effect of covenant intensity is more pronounced when the creditors have higher bargaining power, when loans outstanding are closer to maturity, and when the conflict of interest between creditors and other stakeholders is greater. Taken together, our findings imply that debt covenants, which are designed to alleviate creditor-shareholder conflicts, may have unintended consequences of exacerbating the conflicts between creditors and other stakeholders. In particular, restrictive loan covenants may pressure managers into having a short-term focus, thereby discouraging borrowing firms from engaging in CSR activities.

Combating the COVID-19 Pandemic: The Role of the SARS Imprint

Abstract

In this essay, we provide evidence of delayed attention and inaction in response to COVID-19 in countries that did not experience SARS in 2003. Using cross-country data, we find that individuals in countries that had SARS infections in 2003 search more intensively for COVID-19-related information on Google in late January 2020, the time of the first known outbreak in Wuhan, China. Early attention to the novel virus, as measured by Google searches, is associated with deeper stock market drops in countries with SARS experience. In contrast, people in countries without SARS experience started to pay more attention much later, in March. Moreover, governments in these countries responded significantly more slowly in implementing social distancing policies to combat domestic COVID-19 outbreaks than governments in countries with SARS experience. Moreover, such early responses of individuals and governments in countries with SARS experience are prevalent within continent, even

in non-Asian countries. Furthermore, people in countries with SARS experience are more compliant with social distancing rules. These timely attention and proactive responses of individuals and governments are more pronounced in countries that reported deaths caused by SARS, which left deeper imprints. Our findings suggest that the imprint of similar viruses' experience is a fundamental mechanism underlying timely responses to COVID-19.

How Do Individual Politicians Affect Privatization? Evidence from China

Abstract

This paper investigates how politicians' patronage connections affect privatizations in China. The connections to top political leaders (i.e., Central Committee of the Communist Party of China) make local politicians engage more in rent-seeking by selling state-owned enterprises (SOEs) at substantial discounts. These connected local politicians are also more protected in anti-corruption investigations, thus extracting more rents by selling SOE assets at substantial discounts. Consequently, the privatizations conducted by the local politicians with patronage connections achieve significantly lower gains in efficiency and performance. To identify the role of patronage connection in privatization, we use the mandatory retirement age cut-offs of Central Committee members in the regression discontinuity design. We find drops in price discounts of privatization deals and jumps in efficiency for privatized SOEs when local politicians lose connections to Central Committee members around the retirement age cut-offs.

Keywords: Patronage Connection, Rent-seeking, Privatization, China

JEL Classification: D73, G30, L33

1. Introduction

Political connections play an important role in economies across many countries (e.g., Fisman (2001)).¹ In particular, patronage connections among bureaucrats and politicians set the foundation of political systems in both democratic and autocratic countries (e.g., Grindle (2012); Xu (2019)). However, theoretical arguments on how patronage affects economic performance are ambiguous, and the empirical evidence is limited.²

This paper fills this gap in the literature by documenting an underlying channel of how patronage induces corruption and protects local politicians from rent-seeking activities in privatizations and by estimating the consequences on the performance of privatized state-owned enterprises (SOEs). We use data from China, where patronage substantially influences the political and economic systems (e.g., Fisman et al. (2020)), and many SOEs have been privatized in past decades.³ We find that, in general, privatized SOEs increase efficiency and productivity, while there is vast heterogeneity in efficiency gains following privatization across individual politicians. The local politicians with patronage connections to the top leaders in the Central Committee of the Communist Party of China (CPC) are protected and engage more in rent-seeking in privatizations, which causes worse performance and efficiency of privatized SOEs.

Our findings contribute to the literature examining how patronage affects economic performance. Despite the large body of the literature on political economy, which starts with Nordhaus (1975) and has been growing ever since⁴, the evidence on the economic consequences of patronage is scant and ambiguous. On the one hand, patronage empowers discretions in public-sector (e.g., Colonnelli, Prem and Teso (2018); Fisman et al. (2018)) and private-sector (e.g., Bertrand (2009)) appointments, which introduce distortions and inefficiencies in economic activities (e.g., Xu (2018, 2019)). Connections to patrons could disincentivize subordinates, which

1 For example, borrowers with political connections enjoy better credit access (e.g., Johnson and Mitton (2003); Khwaja and Mian (2005); Faccio (2006); Faccio, Masulis and McConnell (2006)).

2 See detailed discussion in Voth and Xu (2020) for “good” and “bad” patronages in terms of economic implications.

3 Kowalski et al. (2013) show that over 10% of the world’s largest firms are state-owned. China has the highest number of SOEs, especially in strategic industries. Many of them rank among the largest corporations across the globe.

4 See, for example, MacRae (1977), Kornai (1979), Alesina and Sachs (1988), Shleifer and Vishny (1994), Biais and Perotti (2002), Sapienza (2004), Dinç (2005), Khwaja and Mian (2005), Cohen, Coval and Malloy (2011), Carvalho (2014). Some studies explore how politicians’ demographics affect government policies and economic activities (e.g., Levitt (1996); Dollar, Fisman and Gatti (2001); Washington (2008)).

leads to worse performance (e.g., Prendergast and Topel (1996)). On the other hand, patrons have better private information to evaluate and select better-performed subordinates (Aghion and Tirole (1997)), who are thus incentivized to exert efforts for performance (e.g., Voth and Xu (2020)). The discussion is far from being closed, and the empirical evidence is limited. We add to this on-going discussion by showing that corruption is one of the fundamental channels underlying patronage's detrimental effects on economic performance.

Specifically, in China, the implementation of privatization is decentralized and conducted mainly by local politicians (Xu (2011)). We manually collect data from the curricula vitae of 1,746 leading city politicians (i.e., mayors and city secretaries of the CPC), across 326 cities in China between 1998 and 2009. Accordingly, we obtain firm-level panel data from the Chinese Industry Census (CIC) and restrict the sample to the local SOEs at the city level or below, which are under the direct jurisdiction of those city secretaries. This sample of local SOEs covers 96.7% of the privatization cases in CIC.

Based on those data, we develop the main hypothesis in the paper: when local politicians have strong patronage connections (e.g., connection to top leaders), they enjoy more privileges, such as better protections by their patrons in anti-corruption campaigns (e.g., Zhu and Zhang (2017)). Consequently, these politicians engage in more rent-seeking activities in privatization, which hurts privatized SOEs' performance.

To test this hypothesis, we measure patronage based on local politicians' connections to the Central Committee members of the CPC. The Central Committee is comprised of approximately 200 full member representatives to the National People's Congress, the highest organ of state power. We define local politicians as connected if they have worked as subordinates for Central Committee members.⁵ For each local politician, the connections are time-varying due to turnover in the Central Committee. Moreover, for rent-seeking activities, we follow Fisman and Wang (2014) to estimate the value loss amount for negotiated transfer privatization deals. In particular, the value loss amount is equal to the market value of the shares transferred in privatization minus the actual selling price (i.e., negotiated transfer price).

⁵ We employ standard approaches in the literature to define connections among politicians (e.g., Jia et al. (2015) and Jiang (2018)). See detailed discussion in Section 2.3.

Based on those data, we first document that although privatizations generally increase SOEs' efficiency, the gains are significantly less pronounced when connected local politicians conduct the privatizations. Specifically, consistent with the conventional wisdom, the multivariate analyses show that, on average, privatized SOEs increase their total factor productivity (TFP), return on assets (ROA), operating return on assets (OROA), and total sales by 6.7%, 15.6%, 6.4%, and 18.1%, respectively. However, compared to unconnected local politicians, the improvements in the TFP, ROA, OROA, and total sales of privatized SOEs are reduced by 42.2%, 62.3%, 75.4%, and 20.7%, respectively, for connected local politicians.

The variation in patronage connections is semi-exogenous. Specifically, for individual local politicians, the change in their connections comes from their former bosses' promotions to the Central Committee. Additionally, the connections among politicians are predetermined by their past working histories. Thus, the variation in patronage connections is not directly linked to local politicians' concurrent actions. However, these connections could still be correlated with other factors that drive both political connections and privatization outcomes. To identify the role of patronage connections, we employ the fuzzy regression discontinuity design (RDD) by using the compulsory retirement age cut-off for the Central Committee members to explore the discontinuities in local politicians' connections and privatization outcomes. In China, the age limit for term renewal is 64 (68) for Central Committee members at the ministerial (national) level.⁶

In the first stage of fuzzy RDD, consistent with the age limit policy, we find that all Central Committee members step down when they are older than their compulsory retirement ages in turnover years. In the second stage of the fuzzy RDD, we regress the changes in efficiency before and after the privatization on whether the local politicians who conduct the privatization lose connections due to the estimated stepping down of their connected Central Committee members. We find that the efficiency gains of privatizations jump significantly when the local politicians lose their connections. For example, the increases in TFP, ROA, and OROA two years after

⁶ In China, the turnover of the Central Committee occurs every five years. The age limit for term renewal of minister-level and national-level Central Committee members are 64 and 68, respectively. This rule of compulsory retirement ages has been strictly enforced without a single exception in our sample period. See a detailed discussion in Section 2.3.

privatization jump by approximately 31%, 74.9%, and 52.9%, respectively, for local politicians who lose connections due to the stepping down of their connected Central Committee members.

To support the local continuity assumption, we repeat the RDD for other local politicians' characteristics (e.g., age, minority ethnicity, education level, and years in office), and none of them have significant changes around the cut-offs of compulsory retirement ages. This mitigates the concern that other local politicians' characteristics correlate with their political connections and drive the jumps in privatization efficiency gains around the cut-off. In addition, the ratio of privatized SOE numbers over total SOE numbers at the city level does not jump at the cut-off either, which mitigates the concern that the connected politicians might conduct more privatization deals and have to choose ones with potential low-efficiency gains. Consistently, the panel regressions of privatization ratios also show no significant associations with patronage connections. This analysis at the extensive margin rules out a mechanical explanation: connected local politicians tend to conduct more privatizations, which lowers down the average efficiency gain. Overall, these results establish the causal effects of patronage connections on privatization implementations and outcomes. As we specifically indicate, the stepping down of Central Committee members due to the age cut-off is predictable, and our identification comes from the discontinuities rather than exogenous shocks.

To further support our hypothesis, we examine fundamental channels underlying such heterogeneity in privatization outcomes. First, we test whether the connected local politicians engage in more rent-seeking activities in privatization deals, given that these rent-seeking activities lead to the worse subsequent operating performance for privatized SOEs (Fisman and Wang (2014)). We find that the selling of SOE assets under connected local politicians are deeply discounted from the market value. Consistent with the panel regression, we find that the level of value losses drops by 12.60% when connected local politicians lose patronage connections due to the retirements of the Central Committee members at the age cut-off in the RDD analysis. This RDD results also capture the effects that local politicians foresee the step down of their connected top leaders, and they increase the rent-seeking activities in privatization deals when they still can.

Second, using data of anti-corruption investigations in China, we find that the likelihood of being investigated is significantly lower for connected local politicians. Even under investigation,

the connected politicians are protected and receive significantly shorter jail time in sentences, while amounts of corruption are significantly higher for those connected local politicians.

Taken together, our findings support the rent-seeking channel underlying the worse performance of privatized SOEs under connected local politicians. When local politicians' patrons step down, they lose protection from top political leaders and thus engage less in rent-seeking, which leads to better privatization outcomes. Our maintained assumption is that Central Committee members' influence is at least reduced if not altogether lost after retirements. For example, Bertrand et al. (2020) discuss career concerns and motivations of bureaucracies around retirement ages. Retirements significantly reduce bureaucracies' influence. In addition, Jenter and Lewellen (2015) show that, in the private sector, retirements can also influence executives' incentives, such as their preferences in acquisitions.

Besides our contributions to the literature in political economy, this paper also adds to the privatization literature. Although the conventional wisdom has shown that privatization leads to improvements in efficiency (e.g., Boardman and Vining (1989); Megginson, Nash and Van Rrandenborgh (1994); La Porta and Lopez-de-Silanes (1999); Megginson and Netter (2001)), this is not always the case since politics play an important role (e.g., Clarke and Cull (2002); Boehmer, Nash and Netter (2005); Dinç and Gupta (2011)). SOEs can be sold in various ways (e.g., Degeorge et al. (2004)), and the efficiency gains depend largely on the methods of privatization implementations, such as new owners and managers of privatized SOEs (e.g., Barberis et al. (1996); Frydman et al. (1999); Fan, Wong and Zhang (2007); Estrin et al. (2009); Gan, Guo and Xu (2017)). Our findings of heterogeneous outcomes of privatizations across patronage networks shed new light on the mixed evidence of efficiency gains from privatization. One closely related study is by Fisman and Wang (2014), which shows that corruption and rent-seeking in the privatization process are prevalent and lead to worse subsequent operating performance.⁷ This paper complements it by showing that the protection from the patron is underlying those rent-seeking activities.⁸

⁷ Other studies have also shown opposing effects of privatizations in China. Jefferson and Su (2006) show that the conversion of SOEs to shareholding enterprises increase the productivity and innovation efforts in China. Sun and Tong (2003) show that privatizations in China improve the earnings ability but not profit returns.

⁸ We also shed light on the puzzle of why incumbent governments want to embrace privatizations. In particular, Shleifer and Vishny (1994) and Boycko, Shleifer and Vishny (1996) argue that governments should have always kept

The rest of this paper is organized as follows. Section 2 describes the institutional backgrounds in China. Section 3 presents the data and summary statistics. Section 4 shows the empirical analysis and results. Section 5 concludes.

2. Background and Hypothesis

2.1. History of SOE Privatization Reform in China

The economic reform opening-up policy in China was started in 1978 by Xiaoping Deng. The privatization of SOEs was one of the most critical parts of the reform, and a major privatization wave started in 1998 under former Prime Minister Rongji Zhu.⁹ In China, SOEs have many privileges and resources from their political connections, especially the large ones in strategic industries such as energy, telecommunications, and finance (e.g., China National Petroleum Corporation, China Mobile, and China Telecom). Although most SOEs are inefficient, banks are still more willing to lend to SOEs due mainly to their soft budget constraints.¹⁰ The lousy performance and enormous losses of SOEs have created an enormous fortune for the Chinese government, especially in the banking sector. It is the primary reason for this privatization wave.¹¹

The primary agenda of this privatization wave is to keep the large SOEs (e.g., central SOEs) untouched while selling small SOEs (e.g., local SOEs) to the private sector. Specifically, in September 1995, the *Ninth Five-Year Plan* and the *2010 Long Range Objectives* were announced in the Fifth Plenary Session of the Fourteenth Central Committee. This plan focused on economic transformation, especially the SOE reform, with the key slogan of “grasp the large and let go of

control rights and preferred higher private ownership for higher bribes. However, empirical evidence shows that governments have been giving up both control and cash flow rights across the globe (e.g., Kikeri, Nellis and Shirley (1992); Boycko, Shleifer and Vishny (1993); Megginson (2005)). See detailed discussions in Hu et al. (2019).

⁹ The first privatization attempts took place from 1978 to 1984, and SOEs started to pay taxes instead of giving their profits directly to the government. This change failed to incentivize SOEs since the tax rate was too high (around 55%), and many SOEs were unable to pay. The second attempt took place from 1984 to 1998. During this period, SOEs contracted out some of their businesses to private sectors. However, most contractors extracted as much rent as possible, which harmed the SOEs and state assets.

¹⁰ For detailed discussions of banks' preference regarding SOEs due to their soft budget constraints, which lead to credit misallocation in China, see, for example, Qian and Roland (1998), Lin and Tan (1999), Cull and Xu (2003), Song and Xiong (2018).

¹¹ Yao (2005) uses a survey data of 800 SOEs from 1995 to 2001 and finds that insolvency was a big problem facing many SOEs, and government and state banks did not want to support these firms anymore. Privatization became the most plausible way out of this problem. There were more than 40 thousand SOEs privatized or re-organized by 1998. Xu (2011) documents that the total loss of SOEs in 1998 was approximately RMB 307 billion in China, which is about 3.7% of annual GDP loss.

the small”.¹² Shares of SOEs are sold in various ways (e.g., sales to private owners, public offerings, joint ventures, leasing). For example, the original purpose of establishing the stock market in China was to fund SOEs. In the CIC data, SOEs controlled approximately 66.27% of the assets in 1998, which dropped to 23.79% in 2009.

During this privatization wave, in addition to the central government, local governments play a key or even a more prominent role in the entire process. In China, each SOE has a rank in the political hierarchy and is under the control of different levels of the State-owned Assets Supervision and Administration Commission (SASAC). For example, municipal-level SOEs are owned by the city-level SASAC and need to give their profits to local municipal governments. Moreover, each SOE has a committee in the CPC, mainly comprised of the SOE’s executive management team (e.g., Chairman and CEO). These people serve as government officials who are accordingly attached to the local governments. In short, local governments and local politicians play a significant role in the progress of privatization. In other words, the implementations of privatization are decentralized and are mainly determined by local politicians (e.g., Xu (2011); Gan, Guo and Xu (2017)).

2.2. Hypothesis Development

In China, factionalism and clientelism play important roles in political and economic systems (e.g., Meyer, Shih and Lee (2016); Huang, Li, Ma and Qian (2017); Fisman et al. (2018)). The main focus of our study is on the role of local politicians’ patronage connections in privatization performance. It is a long debate in the literature on whether patronage connections are good or bad for performance. On the one hand, patronage could hurt economic performances due mainly to distorted appointments in governments. For example, Grindle (2012) documents that patronage increases corruption in appointments of public officials. We hypothesize a particular channel of the detrimental effects of patronage on privatization outcomes in China via rent-seeking.

¹² See the detailed document of the Ninth Five-Year Plan and the 2010 Long Range Objectives at http://www.gov.cn/test/2008-04/21/content_950407.htm. Additionally, Hsieh and Song (2015) described the institutional background of the state sector transformation and found that the government retained control of the large SOEs while privatizing the smaller ones.

***Hypothesis:** When local politicians have strong patronage connections (e.g., connection to top leaders), they are more protected in the system. Consequently, these politicians engage in more rent-seeking activities in privatization, which hurts privatized SOEs' performance.*

More specifically, the literature has shown that connections to the top leaders in China give local politicians more privileges (e.g., Shih, Adolph and Liu (2012)). For example, the connected local politicians are more protected from anti-corruption campaigns (e.g., Zhu and Zhang (2017)). These lead to more corruption activities during the privatization processes. The corruption activities are detrimental for the performance of privatized SOEs since the efficiency gain from privatizations could be erased by rent-seeking activities, which are value-destroying (e.g., Fisman and Wang (2014)).

2.3. Politician Patronage Connections and Retirement Age Cut-offs

To formally test our hypothesis, we explore the variation in connections to top political leaders across individual city politicians. In particular, a city politician is considered powerful and has a strong political background if she is connected to Central Committee members. The Constitution of the CPC states that the National Congress (i.e., Party Congress) is the highest organ of state power, and the Central Committee is the leading body. The Central Committee members vote and elect the most prominent politicians in China, such as the general secretary (e.g., President Xi Jinping), the members of the Politburo, and the members of the Central Military Commissions. Moreover, the members of the Central Committee usually hold high ranking government and party positions, such as chief provincial leaders, ministerial politicians of the State Council, and commanders of military-regional level organizations. The Central Committee has approximately 200 full members and 150 alternate members in recent terms (i.e., from the 15th to the 18th Central Committees), and each term lasts five years. Alternate members fill empty seats on the Central Committee according to the number of votes by which they were elected.

We define a city secretary or mayor as connected if she has worked as a subordinate for at least one member of the Central Committee. For example, the connection between the city politician A (junior politician) and the Central Committee member B (senior politician) is formed when the junior politician A served as subordinate to senior politician B. Moreover, following Jia et al. (2015), the junior politician's rank should be within two levels below the senior politician to

make sure that they indeed work together. To further reduce measurement errors, we restrict the senior politicians to provincial and city secretaries who are the top politicians in the province and city with the discretions to appoint their subordinates and require that the junior politician begins in her position after the senior politician begins in hers (Jiang (2018)). In other words, when a senior politician selects or promotes certain junior politicians to work for her, they are closely connected. In China, it is common for provincial and city secretaries to pick certain junior politicians as their subordinates.¹³ We identify such connections using 26,482 observations of working experience of 1,746 city politicians. For each junior politician, the connections are time-varying, depending on the changes in Central Committee members over time; Central Committee members are elected every five years. The connection comes into effect when the senior politician became the Central Committee member. The connection expires when the senior politician steps down as a Central Committee member.

For promotion to the Central Committee, age is one of the most crucial factors. Specifically, the members of the Central Committee are generally at the ministerial level, such as ministers and provincial secretaries. There is a strict compulsory retirement age for minister-level provincial politicians in China. According to “*Leading Cadres Retirement Provision*”, minister-level politicians cannot renew their term if they are 64 years old or older.¹⁴ Moreover, the Politburo of the Central Committee has approximately 25 members who are ranked at the national level, and their age limit for term renewal is 68 years old. To be precise, national (ministerial) politicians such as provincial secretaries cannot renew their Central Committee membership if they are 68 (64) years old or older in the year of turnover. Figure 1 plots the distribution of start-term ages for Central Committee members at the ministerial and the national levels in our sample period, respectively. We find no single exception to this policy that is strictly enforced.

[Place Figure 1 about here]

3. Data and Summary Statistics

¹³ For example, if city secretary A and her subordinate B worked in the same city, we define the two as connected. When A becomes a member of the Central Committee, and B becomes a city secretary, we consider B has patronage connections to the Central Committee members.

¹⁴ See the detailed document at <http://cpc.people.com.cn/GB/64162/71380/102565/182144/10994167.html>

We utilize two datasets for our empirical analyses. The first one is the firm-level data for privatizations. The second one is the hand-collected politician profile dataset.

3.1. Firm-level Privatization Data

The first dataset we use in this paper is the Chinese Industrial Census (CIC) collected by the Chinese National Bureau of Statistics (NBS). It covers all the manufacturing firms in China, with annual sales of more than USD 700 thousand between 1998 and 2009. This period captures the entire privatization wave under Prime Minister Rongji Zhu from 1998 to 2005 and several years after the wave, which allows us to study the privatization decisions and subsequent outcomes. The data record yearly accounting statements (e.g., balance sheet, income statement, and cash flow statement) as well as other firm characteristics (e.g., number of workers, industry classification, physical location, registration type, political hierarchy, government subsidy, wages, and shareholders) for each firm. In total, we have 706,976 firms, which comprised about 40% of industrial output in China. To our knowledge, CIC is the most detailed database on Chinese manufacturing firms with comprehensive information and sufficient quality.

We use two methods to classify the SOEs and private firms in the CIC. First, we use the firm registration type and define a firm as SOE if it is owned by a government department or collective-owned based on the official NBS classification. For each SOE, we can trace the changes in its registration types over time to identify the privatization timing. Since we are examining how city leaders affect privatization, we focus on firms that are not subject to the central or provincial government. We exclude firms that cannot be matched to sub-provincial or prefectural cities and have non-consecutive records due to missing observations. There are 113,682 SOEs in the sample, and 28,411 of them are privatized. Second, we use the shareholders' information to classify SOEs vs. private firms. In particular, CIC data disclose the shareholdings by five types of owners: state ownership, collective ownership, individual ownership, corporation ownership, and foreign ownership. We calculate the percentage of state ownership by combining the first two. This captures the dynamics of privatizations for individual SOEs since there are many partial privatizations in China (i.e., the state sells part of the SOEs to the private sector, and it usually takes several sales to fully privatize a SOE.)

Moreover, we then obtain the transaction-level data on negotiated transfer deals for listed SOEs from CCERData. This dataset contains details of all negotiated transfer deals from February 1995 to December 2019. For each deal, the data record the date when the transaction was first announced, transferred price, the number of shares transferred, names and identities of the seller and the buyer, the name and stock code of the company whose shares were to be transferred. We download the financial information of those listed SOEs from CSMAR, such as total assets, leverage, ROA, and stock turnover. To be consistent with the CIC sample period, we keep the negotiated transfer deals up to December 2009. Following Fisman and Wang (2014), we calculate the value loss amount as the market value of the shares transferred in privatization minus the actual selling price (i.e., value loss amount = (market trading price - actual selling price) × total shares transferred). To capture rent-seeking activities, we exclude deals that are not finished up to now or have negative value losses from the sample.

3.2. Politician Profile Data

The second data record the politician profiles in mainland China from 1998 to 2009. It covers all the mayors and secretaries of CPC across 326 cities in China and all the provincial governors and secretaries for 32 provinces in China. We get the name list and the biographic information of these politicians from the China Stock Market & Accounting Research (CSMAR) Database. However, the quality of politicians' profiles might vary among different cities, especially for small ones. We cross-validate the data using information from CSMAR and Baidu Encyclopedia. Specifically, we manually search for politicians' curriculum vitae (CV) from the Baidu Encyclopedia, a Chinese-language collaborative web-based encyclopedia provided by the Chinese search engine Baidu. Baidu Encyclopedia is the top Chinese online encyclopedia and generally offers extensive backgrounds of famous people (e.g., politicians).

This profile dataset covers 1,746 leading city politicians (i.e., mayors and city secretaries of the CPC). Each CV records the politician's gender, age, history of education, place of birth, work experience. Moreover, in China, it is common for people of the same name. We double-check the politicians who have the same name and give them unique IDs. We obtain the complete working histories of politicians from their CVs and complements our data by the Chinese Political Elite Database (Jiang, 2018). Moreover, the data also record the anti-corruption consequences for local politicians. Specifically, for individual politicians, we know whether they have been under

investigations in anti-corruption campaigns, and the outcomes of their investigations (e.g., convictions, corruption amounts, including bribes received and political graft, and punishments, such as demotions, length of jail sentences, and death sentences).

We merge the firm and politician data. In particular, the CIC records an 11-digit number that can locate a firm at the street level. We cut the first four digits to identify the city and use it to match city politicians at the city level.¹⁵ Moreover, the CIC data record firms' ranks in the political hierarchy so that we can observe whether a firm is under the jurisdiction of the central government, provincial governments, city governments, or below.

As discussed in Section 2.1, the main goal of the privatization wave is to sell the small local SOEs while keeping the large SOEs (e.g., central SOEs) state-owned (i.e., “grasp the large and let go of the small”). Consistent with this policy, in the CIC data, there are only 1,189 privatizations for central and provincial SOEs, while 34,846 SOEs at the city level or below have been privatized between 1998 and 2009. In other words, 96.7% of privatization cases are from SOEs at the city level or below. In total, RMB 3,656 billion SOE assets have been privatized, and around two-thirds of them are from local SOEs at the city level or below from 1998 to 2009. Over this period, the total assets of central and provincial SOEs increased from RMB 3,800 billion to RMB 8,309 billion, while the total assets of local SOEs decreased from RMB 3,404 billion to RMB 2,418 billion. Our study mainly focuses on the role of city politicians, and we restrict the sample to the SOEs at the city level or below, which covers most privatization activities in China.

3.3. Summary Statistics

Figure A.1 shows the time trend of the privatization wave in China. The left panel shows the number of privatized SOEs over time. Consistent with the privatization agenda, the major wave of privatization is during the early 2000s. After 2005, the pace has been slowing down. The right panel shows the total assets privatized each year and have similar patterns as the number of privatized SOEs. In Figure A.2, we listed the percentages of SOEs that have been privatized across industries. Most of the privatization activities concentrate on strategic industries such as

¹⁵ In total, we have 326 cities in the sample. We exclude Beijing, Tianjin, Shanghai, and Chongqing, which are province-level municipalities under the direct control of the state council. These four cities are classified as provinces.

energy, chemical, and mining. The textile industry was also heavily targeted by the privatization wave.

Table I is the summary statistics of the main variables in the regression sample in this paper. Panel A is for CIC data. In total, there are 706,976 individual firms in CIC data from 1998 to 2009. To investigate how city politicians affect privatization, we restrict the sample to city-level SOE (i.e., exclude SOEs owned by central or provincial governments). We further restrict the sample to the firms that first appeared in our database as an SOE and drop firms that have non-consecutive records over the years. Firms located in the four province-level municipalities are excluded (e.g., Beijing, Shanghai, Tianjin, and Chongqing). Our sample has 507,446 firm-year observations. The dummy *Private* is for whether the SOEs have changed the registration types from state-owned to privately owned. This is the primary variable we use to measure privatization progress.¹⁶ On average, 22.5% of the SOEs have been privatized between 1998 and 2009. We also use the variable *Private Share* to measure privatization progress, which is the percentage of private ownership of SOEs. On average, 33.8% of the shares are sold to private sectors. Moreover, for our main efficient measurements, the average *TFP*, *ROA*, and *OROA* are 1.421, 5.294, and 10.291, respectively. In particular, we calculate the TFP following Cobb-Douglas form, which is the residual in the regression of the log(total revenue) against log(total assets) and log(total employment) in the panel of firms in the CIC survey from 1998 to 2009.

Table I Panel B is for politician data at the city-year level. In China, the secretary of CPC is the top politician in cities, and we focus on them in the analyses. There are 3,706 observations with 1,048 unique city secretaries over 326 cities from 1998 to 2009. On average, the age of city secretaries is approximately 51.2. 97.9% of them are male, and 53.5% of them have obtained master's degrees or above. Moreover, 7.6% of them have minority ethnicities (i.e., other than Han). Moreover, the mean of *Connection* is 0.593, which means that 59.3% of the city secretaries are connected to at least one Central Committee member. The mean of *Connection#* is 0.659 with the

¹⁶ There are few cases where the registration types of privatized SOEs revert to state-owned and then become private-owned again. The reverse of the privatization in China is documented in Huang et al. (2017). We use the last change from state-owned to private-owned as the privatization time. For robustness check, we use the first change, and the results are similar.

maximum value of 4. This means that city secretaries can be connected to at most 4 Central Committee members.

Panel C is for negotiated transfer deals for listed SOEs' privatizations. Our sample has 1,673 deals across 536 SOEs. 72.3% of deals were sold to private-owned enterprises. On average, each deal transferred 13% of the total share outstanding. Moreover, on average, RMB 208 million are lost per deal, and 71.8% of total values are lost throughout our sample.

[Place Table I about here]

4. Empirical Analysis and Results

4.1. Privatization Outcomes and Individual Politicians

We start the empirical analyses by looking at the privatization outcomes, especially the heterogeneity across local politicians. In particular, we restrict the sample to firms that have been state-owned and perform the OLS regressions of SOE activities and several performance measurements on privatization progress. Formally, the regression can be expressed as follows:

$$Y_{i,t} = \alpha + \beta \times Private_{i,t} + \gamma \times Control_{i,t} + \eta_i + \zeta_t + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ represents the measurements of the efficiency of firm i in year t , such as TFP , ROA , $OROA$, and other firm activities such as sales and market share. $Control_{i,t}$ represents firms' characteristics, such as total sales or total assets. The main variable of interest is the dummy $Private_{i,t}$, which represents whether firm i is registered as a private firm or not in year t . In other words, when $Private_{i,t}$ changes from zero to one in year t , the SOE is privatized in that year. We control for firm fixed effects to use the variation within the firm and control for year fixed effects to condition out the macro time trend. The sample is a panel at the firm×year level, and the standard errors are clustered at the firm level.

Table II Panel A shows the regression results. From columns (1) to (5), the coefficients of *Private* are all significantly positive, suggesting that privatized SOEs increase their efficiency and business activities. For example, for TFP in column (1), the coefficient of *Private* is 0.067 at the 1% significance level, which means that the TFP of SOEs increases by 6.7%. We also find the significant positive coefficients of *Private* in columns (2) and (3) for ROA and $OROA$, respectively, suggesting that the gains from privatization are robust across measurements of firm

efficiency. Moreover, the coefficients of *Private* in columns (4) and (5) for *Log(Sales)* and *MktShare* are also significantly positive. Therefore, the performance of privatized SOEs also improves significantly, which is consistent with the efficiency gains shown in columns (1) to (3). In Panel B, instead of dummy *Private*, we use *Private Share* as the independent variable, which is the percentage of shares owned by the private sector. Consistent with Panel A, the coefficients of *Private Share* have significantly positive coefficients on *TFP*, *ROA*, *OROA*, *Log(Sales)*, and *MktShare*. Overall, these findings suggest that, as in many other countries, privatization improves SOEs' efficiency and performance in China.

[Place Table II about here]

Next, we explore the heterogeneity in the outcomes of privatizations across individual local politicians. As discussed in Section 2.1, the implementation of privatizations is decentralized in China. As top politicians in cities, city secretaries are influential so that we focus on the effects of city secretaries' patronage connections on privatization outcomes.¹⁷ To formally test it, we interact the dummy *Private* with the dummy *Connection* in the panel regressions, as in Table II, where *Connection* is the dummy for whether the privatization is in the city where the city secretary is concurrently connected to full Central Committee members. Since firm fixed effects are controlled in all regressions, the time-invariant *Connection* is thus subsumed. Table III shows the results. Consistent with Table II, the dummy *Private* has significantly positive coefficients on *TFP*, *ROA*, *OROA*, *Log(Sales)*, and *MktShare*. Moreover, in column (1) of Panel A, the coefficient of *Private* $\times$ *Connection* is -0.038 and is significant at the 1% level, suggesting that the efficiency gain in *TFP* after privatization is significantly less if a connected city secretary conducts the privatization deal. On average, the improvement in the *TFP* of privatized SOEs under unconnected city secretaries is 9.0%, while the increase in *TFP* is only 5.2% under connected secretaries, which is equivalent to an approximately 42.22% decrease in efficiency gains. In columns (2) and (3) of Panel A, the coefficients of *Connection* $\times$ *Private* are -0.828 and -0.908, respectively, and are both significant at the 1% level. This means that the efficiency gains of *ROA* and *OROA* after privatization are significantly less if a connected city secretary conducts the privatization. In Panel B, we interact the dummy *Private* with *Connection*#, and the results are similar to those in Panel A.

¹⁷ In unreported results, we include city mayors' patronage connections and find similar results.

[Place Table III about here]

The literature has pointed out the issues in measuring TFP (e.g., Beveren (2012), Imrohoroglu and Tuzel (2014)). As a robustness check, we use various measurements of TFP and repeat the analyses. In particular, we recalculate TFP in two ways: 1) controlling industry and year fixed effects; 2) using the value-added amount as the dependent variable. The results are in Table A.2 in the Internet Appendix, which are similar to Table III: the TFP generally increases for privatized SOEs, but significantly less so for the deals conducted by connected politicians.

Additionally, we run the cross-sectional regressions of changes in SOE performance before and after the privatization on the city secretary's political connections. Consistent with Table III, we find that privatizations implemented by connected city secretaries are significantly worse than those implemented by unconnected city secretaries. In Appendix Table A.3, we calculate ΔTFP , ΔROA , $\Delta OROA$, one year before and after the privatization in columns (1) to (3). In Panel A, the coefficients on *Connection* are negative at the 1% significance level. The results still hold if we expand the window to two years of average change before and after privatization in columns (4) to (6). Again, we find similar results for *Connection#* in Panel B.

In summary, we find suggestive evidence in Tables III that although privatization is associated with efficiency increases on average, these benefits of privatization are less pronounced when local politicians with connections to top leaders conduct the privatization. Consistent with Xu (2011), our findings suggest that privatization implementations are decentralized in China, and local politicians play an important role in the process.

4.2. Regression Discontinuity Design

In Tables III, we use the variation in the local politician's connections, which is time-varying. A city secretary can be considered as connected if a senior official to which she is connected is elected as a member of the Central Committee, while she might also lose the connection if the connected senior politician steps down. The connection between a local politician and a senior politician is based on their previous work experience. This variation is semi-exogenous since the variation in connections is not directly related to the city leader's concurrent action. However, this approach does not entirely rule out other factors that could drive both the city leaders' connections and privatization outcomes. For example, the connected local politicians could be assigned to more developed cities with relatively more efficient SOEs than the unconnected politicians, which

explains the lower efficiency gains of the privatizations conducted by the connected local politicians.

To identify the causal effect of patronage connection on privatization outcomes, we use the mandatory retirement age cut-off for members in the Central Committee to explore the discontinuities in local politicians' connections and privatization outcomes in the setting of fuzzy regression discontinuity design (RDD). As discussed in Section 2.3, the age limit for Central Committee member appointments is strictly enforced. In particular, the age limit for term renewal for a national (ministerial) politician is 68 (64) years old.

Based on those the compulsory retirement cut-off ages, we employ a fuzzy RDD since even if a politician is younger than the retirement age, the term renewal is not guaranteed. Formally, the RDD regressions can be expressed as follows:

$$LosePower_{it} = \beta_{0r} + \beta_{1r} \times Distance_{it} + D_{it}[\beta_{0l} + \beta_{1l} \times Distance_{it}] + \varepsilon_{it} \quad (2)$$

$$Y_{jit} = \beta_{0r} + \beta_{1r} \times Distance_{it} + Lose\widehat{Power}_{it}[\beta_{0l} + \beta_{1l} \times Distance_{it}] + \varepsilon_{it}, \quad (3)$$

where Equation (2) illustrates the first stage in the fuzzy RDD, and Equation (3) illustrates the second stage. The outcome variables Y_{jit} is the change in various efficiency measurements (i.e., ΔTFP_2 , ΔROA_2 , and $\Delta OROA_2$) following the privatization of firm j , which is privatized in year t by city secretary i . The regression is cross-sectional at the firm level, and we restrict it to privatizations conducted by city secretaries who were ever-connected with Central Committee members. For independent variables, $Lose\widehat{Power}_{it}$ is estimated from the first stage outcome variable $LosePower_{it}$, which is a dummy that is equal to one if city secretary i is connected to a senior politician who has stepped down in year t when firm j is privatized. $Distance_{it}$ is the difference between city secretary i 's connected senior politician's start term age and the cut-off age.¹⁸ For example, when city secretary i 's connected senior politician serves in the Central Committee in year t , the senior minister-level (national-level) politician's age when she started this term must be below 64 (68), and $Distance_{it}$ is negative. In contrast, when city secretary i 's connected senior politician does not serve in the Central Committee at the year t , the senior

¹⁸ When calculating the distance, we minus the senior politician's start term age by their retirement age cut-offs (i.e., 64 for minister-level politicians and 68 for national-level politicians).

politician's age when she started the term might be higher than or equal to the cut-off age (has stepped down from the Central Committee), and $Distance_{it}$ is positive. D_{it} is a dummy variable that takes the value of one if $Distance_{it}$ is positive and zero for negative $Distance_{it}$. The coefficients β with subscripts r and l stand for estimations on data exclusively to the right and left of the cut-off age, respectively.

Figure 2 plots the probability of treatment (i.e., step-down) around the age cut-offs. We show that around the age cut-off, which is zero in the horizontal axis in Figure 2, top politicians' stepping down (i.e., treatment) probability jumps from almost 0 to 1. This is consistent with Figure 1, which shows that none of the minister-level (national-level) Central Committee members could renew another term when they are older than or equal to 64 (68) years old in the turnover year. Moreover, the probability of stepping down is not precisely zero for young politicians since they might not renew their term for other reasons, such as being investigated by anti-corruption campaigns.

[Place Figure 2 about here]

In Figure 3, we plot the privatization outcomes against the distance of connected senior politicians' start-term ages to their age cut-offs (point zero in the horizontal axis). In particular, we plot the changes in TFP , ROA , and $OROA$ two years before and after privatization (i.e., ΔTFP_2 , ΔROA_2 , and $\Delta OROA_2$). We find significant jumps in TFP gains following privatization at the cut-off when the local politician's connected Central Committee members step down due to the age limit. In Figure 3, we also plot the 95% confidence intervals of the linear best fits for the observations in the right and left sides of the cut-offs, respectively. The 95% confidence intervals for the right and left sides do not overlap at zero, which means that the jumps in TFP gains following privatization at the cut-offs are statistically significant. Furthermore, we find similar jumps for ROA and $OROA$ at the cut-off, which further strengthens our identifications.

[Place Figure 3 about here]

In addition to the unconditional patterns in Figure 3, we employ the fuzzy RDD to run the nonparametric local linear regression as in Equations (2) and (3). Table IV reports the conventional estimation and the bias-corrected estimation by Calonico, Cattaneo, and Titiunik (2015). Consistent with Figure 3, in column (1), the local Wald estimators are 0.306 and 0.319 at the 5% significance level for the conventional estimation and the bias-corrected estimation, respectively.

This suggests that the average improvement in *TFP* two years before and after privatization jumps by approximately 31% if the city secretary is connected to a senior politician who is just above the age of retirement and has stepped down from the Central Committee. Column (2) shows the average improvement in *ROA* two years before and after privatization. The local Wald estimators are 3.969 and 3.880 at the 5% significance level for the conventional estimation and the bias-corrected estimation, respectively. In column (3), we use the average improvements in *OROA* two years before and after privatization, and the conventional local Wald estimators are 5.447 and 5.657 at the 10% significance levels, respectively. For robustness checks, we repeat the RDD regressions with 100%, 200%, and 300% optimal bandwidth, respectively, following Imbens and Kalyanaraman (2012) and find similar significant jumps in Table A.4.

[Place Table IV about here]

The stepping down of Central Committee members due to the age cut-off is predictable. Specifically, our identification in the RDD approach comes from discontinuities rather than exogenous shocks. The key identification assumption is local continuity. In particular, local continuity requires that all factors other than the treatment variable vary continuously at the age cut-off. In Figure 4, we plot several politician characteristics, such as local politicians' *Age*, *Gender*, *Minority*, *Education*, and *Tenure* against the distance of their connected senior politician's start term age to the age cut-off. In all the graphs, we do not find any significant jumps around the cut-offs. This suggests that the privatization performance jumps documented in Table IV are not driven by the city secretary's personal characteristics other than the connection.

Moreover, we also plot the ratio of privatized SOE numbers over total SOE numbers (*SOERatio*) at the city level against the start term age of the Central Committee members connected to the city secretaries. This mitigates the concern that the connected local politicians might have smaller pools of SOEs to pick from for privatization and have to choose SOEs with low-efficiency gains. Consistently, Table A.5 in the Internet Appendix shows the results for the panel regressions of privatization ratios on patronage connections. There is no significant association between the two. This analysis at the extensive margin rules out a mechanical explanation: connected local politicians tend to conduct more privatizations, which lowers down the average efficiency gain.

[Place Figure 4 about here]

Furthermore, we repeat the RDD regressions of these other factors (i.e., *Age*, *Gender*, *Minority*, *Education*, *Tenure*, and *SOERatio*) in Table V. The conventional estimation and the bias-corrected estimation by Calonico, Cattaneo and Titiunik (2015) are used in the regressions. Consistent with Figure 4, none of the coefficients are statistically significant under the conventional estimation and biased-corrected estimation.

[Place Table V about here]

In summary, we establish the causal effects of local politicians' patronage connections on privatization outcomes. In particular, patronage connections to powerful senior politicians in the Central Committee lead to worse efficiency gains following privatization. Consistently, when local politicians lose connections to top political leaders, they conduct privatizations with better outcomes.

4.3. Rent-seeking Underlying Worse Privatization Outcomes

In the sections above, we show that local politicians with patronage connections conduct privatizations with worse outcomes. In this section, we aim to understand the fundamental reasons underlying these worse privatization outcomes. Specifically, in Section 2.2, we hypothesize that connected local politicians are protected and thus engage more in rent-seeking in privatization, which leads to worse efficiencies of privatized SOEs.

4.3.1. Value Loss in Privatizations

Fisman and Wang (2014) show that the stealing from selling SOE assets at a discounted price is prevalent in China's privatizations, and this rent-seeking behavior leads to the worse subsequent operating performance of privatized SOEs. We follow their approach and calculate the discounts for each privatization deal, as described in Section 3.1.

In particular, we perform the regression of the natural logarithm of value loss amounts (i.e., variable *ValueLossAmount*) on patronage connections for a subsample of 1,537 privatization deals for listed firms in China and report the results in Table VI. In columns (1) to (3), the coefficients of dummy *Connection* are all significantly positive. These estimates suggest that patronage connections are positively associated with the value losses in the selling of SOE assets in privatizations. For example, the coefficient on *Connection* in column (1) is 0.236 at the 5% significance level, suggesting that the discounts in SOE assets selling are 23.6% deeper under

connected local politicians than unconnected politicians. Patronage connections encourage local politicians to extract more rents (i.e., higher amounts of value losses) from privatization deals. In columns (4) to (6), the coefficients on *Connection#* are also significantly positive. We control for multiple deal-level characteristics, such as *PrivateBuyer*, *FractionTransferred*, *TurnOver*, *Log(Asset)*, *Leverage*, and *ROA*.

In addition, we repeat the RDD and perform the regressions of value loss in Panel B. Consistent with OLS regression results, the local Wald estimators are -2.316 and -2.477 for the conventional estimation and the bias-corrected estimation, respectively. This suggests that the level of value losses drops by 12.60% (2.316/18.388) when connected local politicians lose their patronage connections due to the retirements of the Central Committee members at the age cut-off. As we stated above, local politicians can foresee the step down of their connected top leaders due to the retirement age caps. Consequently, they could engage in more rent-seeking activities by selling SOE assets at deeper discounts when they still can, which is captured by the RDD results in Table VI.

[Place Table VI about here]

In summary, the results in Table VI suggest that corruption activities are more severe in the privatizations conducted by the local politicians with patronage connections to the top political leaders. Corruption serves as a fundamental channel underlying the worse economic performance induced by patronage.

4.3.2. Protections from Patronage

To further understand how patronage encourages rent-seeking activities, we explore the protection effects of patronage connections. In particular, we obtain the data for the anti-corruption investigation cases in China and collect the information on the corruption amount and punishment of each anti-corruption investigation of city leaders. In Table VII, we perform the cox proportional duration regression of investigation probability for local politicians. In columns (1) to (4), the coefficients on *Connection* and *Connection#* are all significantly negative. This suggests that the patronage connections are associated with lower likelihoods of investigations under anti-corruption campaigns. In other words, the connected local politicians are protected by their senior politicians in the Central Committee.

[Place Table VII about here]

In addition, in Table VIII, we regress the corruption amount reported in the investigation records on local politicians' patronage connections. In columns (1) to (4), the coefficients of *Connection* and *Connection#* are all significantly positive. For example, in column (3), the coefficient on *Connection* is 0.546 at the 10% significance level, suggesting that the amount of corruption is 54.6% higher for connected local politicians than unconnected ones. This suggests that connected local politicians use their political power to do more corruption activities and extract more rents.

Furthermore, in columns (5) to (8), we perform regressions on the punishments received after the anti-corruption investigations. The outcome variable is the number of years sentenced in jail normalized by the total amount of corruption (i.e., $\text{Log}(\text{Sentence}/\text{Corruption})$). The coefficients of *Connection* and *Connection#* are all significantly negative. For example, in column (7), the coefficient on *Connection* is -0.679 at the 5% significance level, suggesting that the normalized length in jail is 67.9% lower for connected local politicians than unconnected ones.

[Place Table VIII about here]

In summary, these findings in Table VIII suggest that the patronage provides protections to local politicians in the anti-corruption campaign. This protection empowers the local politicians and encourage them to engage in more rent-seeking activities, as shown in Table VI.

5. Conclusion

This study examines the role of politicians in privatization outcomes in the context of China. For the first time, we show that patronage connection is essential for the efficiency gains from privatization. This leads to vast heterogeneity in outcomes of privatizations. In particular, patrons (i.e., Central Committee members) protect their clients (i.e., local politicians) from anti-corruption investigations, thus encourages clients' rent-seeking activities in privatization deals. This hurts the ex-post performance of the privatized SOEs.

Patronage is widespread not only in China but also in many other countries across the globe. Our findings shed new light on the debate about patronage's impacts on economic performance. Our implication is beyond privatization since corruption is prevalent in many other areas in the society (e.g., Shleifer and Vishny (1993)), and patronage is one of the fundamental drives for corruption.

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Figure 1: Start Term Age Distribution. This figure presents the age distribution of Central Committee members at different levels in term 16th to term 18th. We focus on politicians who are not in the military track. The top panel plots start term age distribution for national-level politicians. The bottom panel is for minister-level politicians. The horizontal axis represents the start term age, and the vertical axis represents the density of the age groups. Start term age is politician's age at the election date.

Figure 2: Probability of Treatment. This figure shows the first-stage fuzzy RDD plot of treatment probability against cut-off adjusted age (age minus retirement cut-off ages, which is 64 for minister-level politicians and 68 for national-level politicians). The vertical axis shows the portability of treatment (i.e., step down) across age groups of Central Committee members. In the horizontal axis, to the right of 0 (including 0) are Central Committee members whose age at the time of term renewal is larger than or equal to the retirement cut-off age. To the left of 0 (excluding 0) are the Central Committee members whose age at the time of term renewal is below cut-off age.

Figure 3 Privatization Consequence. This figure shows the graphical results of the RDD on the privatization consequences (i.e., TPF, ROA, and OROA). The vertical axis denotes the firm's privatization outcomes, as in Table IV. In the horizontal axis, to the left (right) of the cut-off are the firms that are privatized by city secretaries who are connected to the Central Committee members with ages lower (higher) than the mandatory renewable age (i.e., 64 for minister-level politicians and 68 for national-level politicians). 95% confidence intervals are drawn around the linear best fit.

Figure 4: Continuity Assumption. This figure presents the graphical check on the local continuity assumption of the regression discontinuity design. In the horizontal axis, to the left (right) of the cut-off are characteristics of city secretaries who are connected to Central Committee members with ages lower (higher) than the mandatory retirement age (i.e., 64 for minister-level politicians and 68 for national-level politicians), such as *Age*, *Gender*, *Minority*, *Education*, and *Tenure*. We also plot the privatization activity (i.e., *SOERatio*) in the city. Detailed variable descriptions are in Table V. 95% confidence intervals are drawn around the linear best fit.

Table I Summary Statistics

This table describes the summary statistics of our sample. Panel A provides the summary statistics of CIC datasets from 1998 to 2009. Firm year panel data are restricted to firms that are not subject to central or provincial governments and that first appeared in our database as a state-owned enterprise. Firms without available prefecture-level or sub-provincial-level city identifier are excluded. Firms located in the four municipalities are excluded (e.g., Beijing, Shanghai, Tianjin, and Chongqing), and cities not covered by our politician datasets are also dropped. In total, there are 113,682 unique firms in the sample, with 507,448 observations across 326 cities. *TFP*, *ROA*, *OROA* are winsorized at 1% at both ends. Panel B provides the summary statistics of prefecture-level or sub-provincial-level city secretary data, which is a city-year panel. City secretary data covers 1,048 unique city secretaries across from 1998 to 2009. Panel C reports the summary statistics for the 1,673 negotiated transfer deals across 536 stocks. See Table A.1 for detailed variable definitions.

Variable	Mean	Observations	S.D.	P25	Median	P75
<i>Panel A: CIC Sample</i>						
<i>Private</i>	0.225	507446	0.418	0	0	0
<i>Private Share</i>	0.338	467015	0.448	0	0	1
<i>TFP</i>	1.421	495876	1.339	0.737	1.582	2.297
<i>ROA</i>	5.294	497223	15.562	-0.047	0.911	6.150
<i>OROA</i>	10.291	497299	17.838	0.962	3.744	10.670
<i>Log(Sale)</i>	8.932	506242	3.464	8.570	9.463	10.496
<i>MktShare</i>	0.017	506242	0.164	0.001	0.003	0.010
<i>Log(Asset)</i>	9.342	506246	2.671	8.563	9.506	10.564
<i>Panel B: Politician Sample</i>						
<i>Connection</i>	0.593	3706	0.491	0	1	1
<i>Connection#</i>	0.659	3706	0.603	0	1	1
<i>Age</i>	51.171	3598	4.076	48	51	54
<i>Gender</i>	0.979	3706	0.142	1	1	1
<i>Minority</i>	0.076	3706	0.265	0	0	0
<i>Tenure</i>	3.090	3705	1.736	2	3	4
<i>Education</i>	0.535	3706	0.499	0	1	1
<i>FiscalExpense</i>	52.461	3691	69.458	16.290	30.800	62.590
<i>FiscalRevenue</i>	30.350	3687	58.504	6.660	13.120	29.310
<i>Panel C: Negotiated Transfer Deals Sample</i>						
<i>ValueLossAmount</i>	18.388	1652	1.395	17.552	18.538	19.356
<i>PrivateBuyer</i>	0.723	1673	0.448	0	1	1
<i>FractionTransferred</i>	0.130	1673	0.119	0.046	0.091	0.187
<i>TurnOver</i>	1.832	1652	1.273	0.878	1.547	2.456
<i>Log(Asset)</i>	20.480	1652	0.897	19.927	20.452	20.937
<i>Leverage</i>	0.249	1647	0.189	0.134	0.230	0.335
<i>ROA</i>	-0.005	1652	0.193	0.005	0.031	0.057

Table II Privatization Consequence

This table presents the results of OLS panel regressions on privatization consequences. The panel is from 1998 to 2009, and the sample is restricted to local SOEs at the city level or below. In Panel A, the main independent variable, *Private*, is a dummy that takes the value of one if the firm is concurrently registered as a private firm. In Panel B, the main independent variable *Private Share* is the share of ownership that is not owned by the state. *Log(Sales)* are controlled in columns of *ROA* and *OROA*. *Log(Asset)* is controlled in the rest columns. Firm and year fixed effects are controlled in each column. See Table A.1 for detailed variable definitions. Standard errors are clustered at the firm level and reported in parenthesis. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Registration Type</i>					
Variables	(1) <i>TFP</i>	(2) <i>ROA</i>	(3) <i>OROA</i>	(4) <i>LogSale</i>	(5) <i>MktShare</i>
<i>Private</i>	0.067*** (0.005)	0.828*** (0.077)	0.656*** (0.094)	0.181*** (0.009)	0.004*** (0.001)
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	495,876	497,218	497,294	506,227	506,227
Adj. R-squared	0.068	0.039	0.052	0.384	0.001
<i>Panel B: Private Ownership</i>					
Variables	(1) <i>TFP</i>	(2) <i>ROA</i>	(3) <i>OROA</i>	(4) <i>LogSale</i>	(5) <i>MktShare</i>
<i>Private Share</i>	0.082*** (0.004)	0.749*** (0.071)	0.558*** (0.088)	0.167*** (0.008)	0.003*** (0.001)
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	462,052	466,784	466,863	467,014	467,014
Adj. R-squared	0.069	0.035	0.050	0.088	0.001

Table III Political Connection and Privatization Consequence

This table presents the results of OLS panel regressions of privatization outcomes implemented by connected politicians versus unconnected politicians. The sample is from 1998 to 2009 and is restricted to local SOEs at the city level or below. *Connection* is a dummy variable that takes the value of one if the firm is privatized by a city secretary who is concurrently connected with at least one full Central Committee members in the year of privatization. *Connection#* counts the number of contemporaneous connections that the city secretary has with Central Committee members in the year of privatization. *Private* is a dummy variable that takes the value of one if the firm is concurrently registered as a private firm. *Log(Sales)* are controlled in columns of *ROA* and *OROA*. *Log(Asset)* is controlled in the rest columns. Firm and year fixed effects are controlled in each column. Time-invariant *Connection* (*Connection#*) is subsumed by the firm fixed effects. See Table A.1 for detailed variable definitions. Standard errors are clustered at the firm level and reported in parenthesis. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Connection Dummy</i>					
Variables	(1) <i>TFP</i>	(2) <i>ROA</i>	(3) <i>OROA</i>	(4) <i>LogSale</i>	(5) <i>MktShare</i>
<i>Private</i>	0.090*** (0.007)	1.328*** (0.129)	1.204*** (0.155)	0.208*** (0.012)	0.004** (0.002)
<i>Connection</i> × <i>Private</i>	-0.038*** (0.009)	-0.828*** (0.160)	-0.908*** (0.195)	-0.043*** (0.013)	-0.001 (0.002)
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	495,876	497,218	497,294	506,227	506,227
Adj. R-squared	0.068	0.039	0.052	0.384	0.001
<i>Panel B: Number of Connections</i>					
Variables	(1) <i>TFP</i>	(2) <i>ROA</i>	(3) <i>OROA</i>	(4) <i>LogSale</i>	(5) <i>MktShare</i>
<i>Private</i>	0.089*** (0.007)	1.267*** (0.119)	1.166*** (0.143)	0.204*** (0.011)	0.004** (0.002)
<i>Connection#</i> × <i>Private</i>	-0.031*** (0.007)	-0.622*** (0.118)	-0.723*** (0.144)	-0.032*** (0.010)	-0.001 (0.002)
Controls	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	495,876	497,218	497,294	506,227	506,227
Adj. R-squared	0.068	0.039	0.052	0.384	0.001

Table IV Regression Discontinuity Design

This table reports firm-level regression results from the fuzzy regression discontinuity design. The sample is restricted to firms that are privatized by city secretaries who have ever connected with Central Committee members. On both sides around the age cut-off, we use the Mean Square Error optimal bandwidth following Calonico, Cattaneo, and Titiunik (2014). Triangular kernel is used in the local linear regression. Two treatment effect estimators are reported: conventional local Wald estimator and bias-corrected estimator proposed by Calonico, Cattaneo, and Titiunik (2015). The assignment variable is distance between the connected Central Committee's start term age and the age cut-off. The treatment variable is a dummy that takes the value of 1 if the connected Central Committee member has completed his final term. The outcome variable is the privatization outcomes at the firm level. ΔTFP_2 , ΔROA_2 , and $\Delta OROA_2$ are the average annual changes in TFP , ROA , and $OROA$ two years before and after the privatization, respectively. See Table A.1 for detailed variable definitions. Standard errors clustered at the local politician level are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)
Variables	ΔTFP_2	ΔROA_2	$\Delta OROA_2$
Conventional	0.306** (0.139)	3.969** (1.940)	5.447* (3.269)
Bias-corrected	0.319** (0.139)	3.880** (1.940)	5.657* (3.269)
Observations	14,469	14,657	14,665

Table V Regression Discontinuity Design (Continuity Assumption)

This table reports city secretary×term level results from the fuzzy regression discontinuity for the personal characteristics and privatization activity. The sample is restricted to city leaders who have ever connected with Central Committee members. On both sides around the age cut-off, we use the Mean Square Error optimal bandwidth following Calonico, Cattaneo, and Titiunik (2014). Triangular kernel is used in the local linear regression. Two treatment effect estimators are reported: conventional local Wald estimator and bias-corrected estimator proposed by Calonico, Cattaneo, and Titiunik (2015). The assignment variable is cut-off adjusted age, which is the connected Central Committee's start term age minus the age cut-off. The treatment variable is a dummy that takes the value of 1 if the connected Central Committee member has completed his final term. The outcome variables are the personal characteristics and the privatization activity in the city, which is governed by the city secretary. *Age* is the local politician's age at the end of the Central Committee term. *Gender* is a dummy variable that takes the value of 1 if the local politician is male, 0 otherwise. *Minority* is a dummy variable that takes the value of 1 if the politician's ethnicity is non-Han, 0 otherwise. *Education* is a dummy variable that equals 1 if the politician's highest educational achievement is higher than or equal to master's degree. *Tenure* counts the number of years the local politician has served in this term by the end of the Central Committee term. *SOERatio* is the ratio of the number of firms privatized over the total number of SOEs in this city. Standard errors clustered at the local politician level are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variables	(1) <i>Age</i>	(2) <i>Gender</i>	(3) <i>Minority</i>	(4) <i>Education</i>	(5) <i>Tenure</i>	(6) <i>SOERatio</i>
Conventional	-0.431 (0.866)	-0.027 (0.030)	0.040 (0.076)	0.146 (0.105)	-0.578 (0.381)	-0.030 (0.027)
Bias-corrected	-0.415 (0.866)	-0.027 (0.030)	0.053 (0.076)	0.142 (0.105)	-0.622 (0.381)	-0.038 (0.027)
Observations	1,486	1,508	1,508	1,508	1,508	1,507

Table VI Political Connection and Rent-seeking

This table reports results from OLS regression and regression discontinuity of value loss amount in negotiated transfer deals implemented by connected politicians versus unconnected politicians. The sample is restricted to deals with positive value loss. Panel A is a deal-level regression with *ValueLossAmount* as the dependent variable. *ValueLossAmount* is the natural logarithm of the value loss amount in the negotiated transfer deal, where the value loss amount is calculated by the average price (mean of opening and closing price) on the announcement day times number of shares transferred times the discount to the average price. *Connection* is a dummy variable that takes the value of one if the city secretary is concurrently connected with at least one full Central Committee members. *Connection#* counts the number of contemporaneous connections that the city secretary has with Central Committee members. *PrivateBuyer*, *FractionTransferred*, *TurnOver*, *Log(Asset)*, *Leverage*, and *ROA* were controlled in each column. Panel B is a city×year level regression on the value loss amount with the regression discontinuity design. We restrict the sample to deals conducted by city secretaries who are ever connected with Central Committee members. We use the Mean Square Error optimal bandwidth on both sides around the age cut-off following Calonico, Cattaneo, and Titiunik (2014). Triangular kernel is used in the local linear regression. Two treatment effect estimators are reported: conventional local Wald estimator and bias-corrected estimator proposed by Calonico, Cattaneo, and Titiunik (2015). The assignment variable is distance between the connected Central Committee's start term age and the age cut-off. The treatment variable is a dummy that takes the value of 1 if the connected Central Committee member has completed his final term. The outcome variable is the *ValueLossAmount* aggregated at the city level. Robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: OLS Regression</i>						
Variables	(1) <i>ValueLoss Amount</i>	(2) <i>ValueLoss Amount</i>	(3) <i>ValueLoss Amount</i>	(4) <i>ValueLoss Amount</i>	(5) <i>ValueLoss Amount</i>	(6) <i>ValueLoss Amount</i>
<i>Connection</i>	0.236** (0.112)	0.200* (0.117)	0.314* (0.169)			
<i>Connection#</i>				0.304*** (0.098)	0.269*** (0.096)	0.333*** (0.122)
Controls	YES	YES	YES	YES	YES	YES
Politician FE	YES	YES	YES	YES	YES	YES
City FE	YES	YES	NO	YES	YES	NO
Industry FE	NO	YES	NO	NO	YES	NO
Firm FE	NO	NO	YES	NO	NO	YES
Observations	1,537	1,534	1,389	1,537	1,534	1,389
Adj. R-Squared	0.639	0.669	0.773	0.640	0.671	0.774
<i>Panel B: Regression Discontinuity</i>						
Variables	(1) <i>ValueLossAmount</i>					
Conventional	-2.316** (0.985)					
Bias-corrected	-2.477*** (0.985)					
Observations	568					

Table VII Political Connection and Investigation

This table presents the results of the Cox proportional duration regression of investigation probability of connected versus unconnected politicians. This sample covers politicians who served as city secretaries and mayors from 1998 to 2009. For each politician, the sample is extended five years after she ends a term as a city leader to incorporate the subsequent investigation after ending her term as a city politician. The origin and failure event are the years when the local politician first appeared in our dataset and the year in which the local politician gets investigated, respectively. *Connection* is a dummy variable that takes the value of one if the city politician is concurrently connected with at least one full Central Committee members. *Connection#* counts the number of concurrent connections that the city politician has with Central Committee members. Politician characteristics (i.e., *Age*, *Gender*, *Education*, *Minority*), city characteristics (i.e., the natural logarithm of GDP) are controlled in all columns. Province fixed effects are further controlled in columns (2) and (4). Robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variables	(1) <i>Investigation</i>	(2) <i>Investigation</i>	(3) <i>Investigation</i>	(4) <i>Investigation</i>
<i>Connection</i>	-0.317** (0.161)		-0.420** (0.178)	
<i>Connection#</i>		-0.330*** (0.116)		-0.425*** (0.131)
Controls	YES	YES	YES	YES
Province FE	NO	NO	YES	YES
Observations	14,192	14,192	14,192	14,192
Chi-squared	22.310	24.980	64.070	66.870
Pseudo R-squared	0.012	0.013	0.033	0.035

Table VIII Political Connection and Corruption

This table presents the results of the OLS cross-sectional regression of corruption amount and investigation consequence of connected versus unconnected politicians. This sample covers politicians who served as city secretaries and mayors from 1998 to 2009 and subsequently got investigated and sentenced. *Corruption* is the natural logarithm of the corruption amount of the investigated politician. Corruption amount is the sum of bribes received and political graft. *Log(Sentence/Corruption)* is the natural logarithm of the ratio between the number of years sentenced and the corruption amount. For politicians that got sentenced with suspended death penalty and life imprisonment, 20 is given for the number of years sentenced. *Connection* is a dummy variable that takes the value of one if the city politician is ever connected with at least one full Central Committee member from 1998 to 2009. *Connection#* counts the maximum number of connections that the city politician has with Central Committee members from 1998 to 2009. Time invariant politician characteristics (i.e., *Gender*, *Minority*), city characteristics (i.e., the natural logarithm of the average GDP in cities led by the politician) are controlled in all columns. Province fixed effects are further controlled in column (3), (4), (7), and (8). Robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variables	(1) <i>Corruption</i>	(2) <i>Corruption</i>	(3) <i>Corruption</i>	(4) <i>Corruption</i>	(5) <i>Ln(Sentence/ Corruption)</i>	(6) <i>Ln(Sentence/ Corruption)</i>	(7) <i>Ln(Sentence/ Corruption)</i>	(8) <i>Ln(Sentence/ Corruption)</i>
<i>Connection</i>	0.637* (1.911)		0.546* (1.768)		-0.805** (-2.382)		-0.679** (-2.173)	
<i>Connection#</i>		0.429*** (2.682)		0.326** (2.159)		-0.486*** (-2.991)		-0.367** (-2.435)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	NO	NO	YES	YES	NO	NO	YES	YES
Observations	124	124	124	124	119	119	119	119
Adj. R-squared	0.085	0.096	0.196	0.199	0.112	0.119	0.213	0.213

How Do Individual Politicians Affect Privatization? Evidence from China

Figure A.1: Number of privatization and privatized assets of local SOEs at the city-level or below over time. This figure illustrates the privatization pattern from 1999 to 2009. The left graph shows the number of privatized firms per year, and the right graph shows the amount of privatized asset per year.

Panel A

Panel B

Figure A.2: This figure shows the local SOEs' privatization progress across industries. Privatized Asset denotes the assets that are privatized within each industry for each period. For each firm, privatized asset equals the privatization dummy times the firm's total asset. Average State-Owned Asset denotes the averaged assets owned by the government within each industry in each period. Asset owned by the government in each firm equals total asset times the percentage share owned by the government. Top 10 industries (sorted by privatized asset) are displayed. Panel A and Panel B illustrate the privatization progress across industries in 1999-2004 and 2005-2009, respectively.

Table A.1: Variable Definitions and Constructions

Variable	Description
<i>Private</i>	Dummy variable that equals one if the firm is concurrently registered as a private enterprise, and zero otherwise.
<i>Private Share</i>	Percentage share that is owned by the private parties, such as individuals, foreign investors, and non-government institutes.
<i>TFP</i>	The residual value obtained from the regression of the natural log of total revenues on the natural log of total assets and the natural log of total employment following Cobb-Douglas form in the panel of all firms in the CIC survey from 1998 to 2009. It is winsorized at 1% at both ends.
<i>ROA</i>	Net income divided by the current year total asset times 100. It is winsorized at 1% at both ends.
<i>OROA</i>	Operating profit divided by the current year total asset times 100. It is winsorized at 1% at both ends.
<i>Log(Sale)</i>	The natural logarithm of the operating income of the firm.
<i>MktShare</i>	The percent of the firm's operating revenues in the entire 2-digit industry across China.
<i>Log(Asset)</i>	The natural logarithm of the total asset of the firm.
ΔTFP_2	Average TFP two years after the privatization minus average TFP two years before the privatization.
ΔROA_2	Average ROA two years after the privatization minus average ROA two years before the privatization.
$\Delta OROA_2$	Average OROA two years after the privatization minus average OROA two years before the privatization.
<i>Connection</i>	Dummy variable that equals one if the city politician is concurrently connected to at least one full Central Committee member of CPC, and zero otherwise.
<i>Connection#</i>	Number of Central Committee members that the city politician is concurrently connected to.
<i>Age</i>	City politician's contemporaneous age.
<i>Gender</i>	Dummy variable that equals one if the city politician is male, and zero otherwise.
<i>Minority</i>	Dummy variable that equals one if the city politician's ethnicity is non-Han, and zero otherwise.
<i>Tenure</i>	The number of years the city politician has been in this position.
<i>Education</i>	Dummy variable that equals one if the city politician is concurrent educational attainment is higher than or equal to a master's degree, and zero otherwise.
<i>FiscalExpense</i>	Fiscal expense of the city government. In 100 million RMB.
<i>FiscalRevenue</i>	Fiscal revenue of the city government. In 100 million RMB.

(To be continued)

Table A.1: Variable Definitions and Constructions - *continued*

Variable	Description
<i>ValueLossAmount</i>	The natural logarithm of the value loss amounts in the privatization deal. The value loss amount is equal to the market value of the shares transferred in privatization minus the actual selling price (i.e., (market trading price - actual selling price)×total shares transferred).
<i>PrivateBuyer</i>	Dummy variable that equals one if the buyer of the share transfer deal is registered as a privately owned enterprise.
<i>FractionTransferred</i>	The ratio between the number of shares transferred in this deal to all outstanding shares.
<i>TurnOver</i>	Average daily turnover of the stock in the last year.
<i>Leverage</i>	The ratio between total borrowings to total assets of the firm.
<i>Corruption</i>	The natural logarithm of the corruption amount of the investigated city politician. Corruption amount is the sum of bribes received and political graft.
<i>Ln(Sentence/Corruption)</i>	Natural logarithm of the ratio between the number of years sentenced and corruption amount.
<i>Investigation</i>	Dummy variable that equals one if the city politician is investigated in this year.
<i>TFP (With Fixed Effects)</i>	The residual value obtained from the regression of the natural log of total revenues on the natural log of total assets and the natural log of total employment following Cobb-Douglas form in the panel of all firms in the CIC survey from 1998 to 2009. 2-digit industry dummies and year dummies are included in the regression. It is winsorized at 1% at both ends
<i>TFP (Value Added)</i>	The residual value obtained from the regression of the natural log of industrial value-added amount on the natural log of total assets and the natural log of total employment following Cobb-Douglas form in the panel of all firms in the CIC survey from 1998 to 2009. 2-digit industry dummies and year dummies are included in the regression. It is winsorized at 1% at both ends.
<i>SOERatio</i>	The ratio of the number of firms privatized over the total number of SOEs in this city.
<i>AssetRatio</i>	The ratio of total privatized assets over total assets of all SOEs in this city.

Table A.2: Robustness Check (Alternative TFP Measures)

This table presents the results of OLS panel regressions on privatization consequences. The panel is from 1998 to 2009 and is restricted to local SOEs at the city level or below. *TFP (With Fixed Effects)* is the residual in the regression of $\text{Log}(\text{Sales})$ against $\text{Log}(\text{Asset})$ and $\text{Log}(\text{Employment})$ in the panel of firms with industry fixed effects and year fixed effects. *TFP (Value Added)* is calculated with the same regression as *TFP (with Fixed Effects)* replacing $\text{Log}(\text{Sales})$ by $\text{Log}(\text{Value-Added})$. *Private* is a dummy that takes the value of one if the firm is concurrently registered as a private firm. *Private Share* is the share of ownership that is owned by the private sector. *Connection* is a dummy variable that takes the value of one if the city secretary is concurrently connected with at least one full Central Committee members in the year of privatization. *Connection#* counts the number of contemporaneous connections that the city secretary has with Central Committee members in the year of privatization. $\text{Log}(\text{Asset})$ is controlled in all columns. Firm and year fixed effects are controlled in each column. See Table A.1 for detailed variable definitions. Standard errors are clustered at the firm level and reported in parenthesis. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Privatization Consequence</i>				
Variables	(1) <i>TFP (With Fixed Effects)</i>	(2) <i>TFP (Value Added)</i>	(3) <i>TFP (With Fixed Effects)</i>	(4) <i>TFP (Value Added)</i>
<i>Private</i>	0.057*** (0.005)	0.062*** (0.007)		
<i>Private Share</i>			0.080*** (0.004)	0.079*** (0.006)
Controls	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	495,876	304,281	462,052	297,398
Adj. R-squared	0.099	0.051	0.099	0.048
<i>Panel B: Political Connection and Privatization Consequence</i>				
Variables	(1) <i>TFP (With Fixed Effects)</i>	(2) <i>TFP (Value Added)</i>	(3) <i>TFP (With Fixed Effects)</i>	(4) <i>TFP (Value Added)</i>
<i>Private</i>	0.081*** (0.007)	0.095*** (0.011)	0.081*** (0.007)	0.093*** (0.010)
<i>Connection</i> × <i>Private</i>	-0.040*** (0.009)	-0.055*** (0.013)		
<i>Connection#</i> × <i>Private</i>			-0.034*** (0.007)	-0.045*** (0.009)
Controls	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	495,876	304,281	495,876	304,281
Adj. R-squared	0.099	0.051	0.099	0.051

Table A.3: Political Connection and Privatization Consequence (Cross-sectional)

This table presents the results of cross-sectional regressions of privatization outcomes implemented by connected politicians versus unconnected politicians. The panel is from 1998 to 2009 and restricts to local SOEs at the city level or below with non-missing observation in the sample period. *Connection* is a dummy variable that takes the value of one if the city secretary is concurrently connected with at least one full Central Committee members in the year of privatization. *Connection#* counts the number of contemporaneous connections that the city secretary has with Central Committee members in the year of privatization. ΔTFP_1 , ΔROA_1 , and $\Delta OROA_1$ are the changes in *TFP*, *ROA*, and *OROA* one year before and after the privatization, respectively. ΔTFP_2 , ΔROA_2 , and $\Delta OROA_2$ are the average annual changes in *TFP*, *ROA*, and *OROA* two years before and after the privatization, respectively. *Log(Average Asset)* are controlled in columns of ΔTFP_1 and ΔTFP_2 . *Log(Average Sales)* is controlled in the rest columns. The fixed effects of privatization year are controlled in each column. See Table A.1 for detailed variable definitions. Standard errors are clustered at the firm level and reported in parenthesis. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Connection Dummy</i>						
Variables	(1) ΔTFP_1	(2) ΔROA_1	(3) $\Delta OROA_1$	(4) ΔTFP_2	(5) ΔROA_2	(6) $\Delta OROA_2$
<i>Connection</i>	-0.026*** (0.008)	-0.416*** (0.142)	-0.640*** (0.174)	-0.033*** (0.009)	-0.732*** (0.171)	-0.919*** (0.206)
Controls	YES	YES	YES	YES	YES	YES
Privatization Year FE	YES	YES	YES	YES	YES	YES
Observations	27,240	27,614	27,639	14,955	15,233	15,239
Adj. R-squared	0.009	0.003	0.005	0.012	0.014	0.016
<i>Panel B: Number of Connections</i>						
Variables	(1) ΔTFP_1	(2) ΔROA_1	(3) $\Delta OROA_1$	(4) ΔTFP_2	(5) ΔROA_2	(6) $\Delta OROA_2$
<i>Connection#</i>	-0.020*** (0.006)	-0.341*** (0.109)	-0.515*** (0.133)	-0.021*** (0.007)	-0.482*** (0.127)	-0.667*** (0.153)
Controls	YES	YES	YES	YES	YES	YES
Privatization Year FE	YES	YES	YES	YES	YES	YES
Observations	27,240	27,614	27,639	14,955	15,233	15,239
Adj. R-squared	0.009	0.003	0.005	0.012	0.013	0.016

Table A.4: Regression Discontinuity Design (Different Bandwidths)

This table reports firm-level results from the fuzzy regression discontinuity. The sample is restricted to firms that are privatized by city secretaries who are ever connected with central committee members (except Politburo members). For a robustness check, on both sides around the cut-off, 100%, 200%, and 300% optimal bandwidths given by Imbens and Kalyanaraman (2012) are used. Triangular kernel is used in the local linear regression. The assignment variable is distance between the connected Central Committee's start term age and the age cut-off. The treatment variable is a dummy that takes the value of 1 if the connected Central Committee member has completed his final term. The outcome variable is the firm-level privatization consequence, as in Table V. ΔTFP_2 , ΔROA_2 , and $\Delta OROA_2$ are the average annual changes in *TFP*, *ROA*, and *OROA* two years before and after the privatization, respectively. Standard errors clustered at the politician level are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Variables	(1) ΔTFP_2	(2) ΔROA_2	(3) $\Delta OROA_2$
Local Wald Estimator 100%	0.310** (0.144)	3.287** (1.456)	6.116 (3.818)
Local Wald Estimator 200%	0.257*** (0.088)	3.402** (1.345)	5.016** (2.002)
Local Wald Estimator 300%	0.181*** (0.058)	3.061** (1.231)	4.671** (1.839)
Observations	14,469	14,657	14,665

Table A.5: Political Connection and Privatization Activity

This table presents the OLS regressions of privatization decisions. The CIC firm-year panel is aggregated into a city year panel. In Panel A, the main independent variable *Connection* is a dummy variable that takes the value of 1 if the concurrent city secretary is connected with at least one central committee member. In Panel B, the main independent variable, *Connection#*, counts the number of connections that the concurrent city secretary possesses with central committee members. In columns (1) and (2) in both panels, the dependent variable *SOERatio* is the ratio of the number of firms privatized over the total number of SOEs in this city. In columns (3) and (4) in both panels, the dependent variable *AssetRatio* is the ratio of total privatized assets over total assets of all SOEs in this city. City and year fixed effects are controlled in each column. *Ln(FiscalExpense)* and *Ln(FiscalRevenue)* are further controlled in the even columns. Standard errors are clustered at the city level and reported in parenthesis. ***, **, * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Connection Dummy</i>				
Variables	(1) <i>SOERatio</i>	(2) <i>SOERatio</i>	(3) <i>AssetRatio</i>	(4) <i>AssetRatio</i>
<i>Connection</i>	-0.103 (0.361)	-0.090 (0.365)	-1.045 (0.644)	-1.023 (0.642)
<i>Ln(FiscalExpense)</i>		0.696 (0.661)		0.821 (1.355)
<i>Ln(FiscalRevenue)</i>		-0.060 (0.656)		-1.240 (1.325)
City Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	3,693	3,673	3,403	3,401
Adj. R-squared	0.173	0.171	0.048	0.048
<i>Panel B: Number of Connections</i>				
Variables	(1) <i>SOERatio</i>	(2) <i>SOERatio</i>	(3) <i>AssetRatio</i>	(4) <i>AssetRatio</i>
<i>Connection#</i>	-0.044 (0.285)	-0.043 (0.287)	-0.735 (0.503)	-0.718 (0.501)
<i>Ln(FiscalExpense)</i>		0.696 (0.663)		0.831 (1.356)
<i>Ln(FiscalRevenue)</i>		-0.063		-1.242

		(0.656)		(1.323)
City Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	3,693	3,673	3,403	3,401
Adj. R-squared	0.173	0.171	0.048	0.048

Debt Contracting and Corporate Social Responsibility: The Role of Bank Loan Covenants

February 2021

Abstract

This paper studies the real and social impacts of debt contracting by exploring the effect of loan covenants on borrowing firms' CSR (corporate social responsibility) performance. We document that firms with higher covenant intensity (i.e., more restrictive covenants) have poorer CSR performance. The endogeneity tests, which include an instrumental variable approach based on supply-side determinants of loan covenants and two quasi-natural experiments respectively based on SFAS 160 and anti-recharacterization statutes, support a causal relation running from covenant intensity to CSR. Further analyses reveal that the negative CSR effect of covenant intensity is more pronounced when the creditors have higher bargaining power, when loans outstanding are closer to maturity, and when the conflict of interest between creditors and other stakeholders is greater. Taken together, our findings imply that debt covenants, which are designed to alleviate creditor-shareholder conflicts, may have unintended consequences of exacerbating the conflicts between creditors and other stakeholders. In particular, restrictive loan covenants may pressure managers into having a short-term focus, thereby discouraging borrowing firms from engaging in CSR activities.

Keywords: Corporate Social Responsibility; Loan Covenants; Covenant Intensity; Short-term Conflict of Interests

JEL classification: G21; G32; G3; M14

1. Introduction

The conflict of interest between creditors and shareholders induces managers and shareholders to engage in value-reducing risk shifting or asset substitution (Jensen and Meckling, 1976; Myers, 1977). While the theory is well established, the real and social impacts of conflicts between creditors and other stakeholders has received little attention from empirical investigations, partly due to the challenge of empirically measuring the conflict of interests between creditors and shareholders (e.g., Chu, 2018). Therefore, it is of particular importance to study how the creditor-shareholder conflict impacts firms' social performance. In this paper, we focus on loan covenant, a powerful tool to mitigate the conflict and reduce agency costs by restricting the behavior of managers in order to align their interests with that of creditors (Jensen and Meckling, 1976; Myers, 1977; Smith and Warner, 1979; Bradley and Roberts, 2015), and study its effect on firms' corporate social responsibility (CSR) performance.

We investigate CSR as it has become a mainstream in the business practice of companies (e.g., Starks, 2009). According to a recent survey by UN Global Compact-Accenture CEO Study in 2019, 99% of CEOs from large companies believe sustainability will be important to firm's future success and 48% of CEOs are implementing sustainability into their operations.¹⁹ Moreover, the total US asset of the socially responsible investing amounts to \$11.6 trillion in 2018, according to the Forum for Sustainable and Responsible Investment (SIF, 2018).

On the other hand, unlike other conventional forms of investment, investments in CSR require a long-term commitment²⁰, as it is an intangible asset that aligns the long-run interest of other stakeholders such as customers, suppliers, employees, local communities, and the society with that of shareholders (e.g., Hart, 1995; Jones, 1995; Deng, Kang, and Low, 2013; Flammer, 2015). Therefore, the contribution to shareholder value in the long run is of less

¹⁹ For details see <https://www.accenture.com/us-en/insights/strategy/ungcceeostudy>.

²⁰ See, for example, Ferrell, Liang, and Renneboog (2016), Flammer and Bansal (2017), Starks, Venkat, and Zhu (2018), Nguyen, Kecskes, and Mansi (2020), among others.

interest to creditors, who are fixed claimants. Hence CSR offers a natural setting to examine the real social impact of creditor-shareholder conflict. Following this argument, we expect loan covenants designed to align the interest between equity holders and debt holders to discourage firms to engage in CSR and thus negatively affect firms' CSR performance.

The reasons are twofold. First, bank loan covenants are designed by the banks to restrict managers' risk shifting behavior and excessive risk taking (Jensen and Meckling, 1976; Myers, 1977; Bradley and Roberts, 2015). For instance, Demiroglu and James (2010) document that tight covenants are associated with declines of investment spending and net debt insurance. Similar to R&D, advertising, and other corporate investments, CSR reduces firms' short-term profits by having large amount of upfront cash outflows for long-run benefits for stakeholders but does not generate immediate gains to debt holders (e.g., Wood and Jones, 1995; Orlitzky, Schmidt, and Rynes, 2003). This creates short-term conflicts of interest between creditors and other stakeholders. Therefore, these debtholder-value-destroying activities are in general constrained by tighter thresholds in covenants. Second, higher covenant intensity largely increases managers' ex ante concerns about the general consequences of technical defaults. Since technical defaults are associated with significant creditor intervention and substantial modifications to loan agreements in terms of higher interest rates and reductions in credit availability (e.g., Beneish and Press, 1993; Dichev and Skinner, 2002; Chava, 2014) and loss of control rights (e.g., Chava and Roberts, 2008; Nini, Smith, and Sufi, 2009; Demiroglu and James, 2010), managers are more incentivized to reduce investments in CSR activities. Overall, we expect tighter covenants to have negative effects on CSR performance, and the effect will be stronger for firms whose creditors are more powerful and whose loans are closer to maturity.

To test these hypotheses empirically, we construct our measures of covenant intensity following Bharath et al. (2011) and Bradley and Roberts (2015) and investigate its effect on firms' CSR performance. Similar to previous studies (e.g., Flammer, 2015, 2018), we measure

firm's CSR performance using the strength score from MSCI ESG KLD STATS DATA SET created by Kinder, Lydenberg, and Domini Research & Analytics (KLD). In particular, we obtain our main CSR measure as the sum of strength scores in the following 6 dimensions: community activities, diversity, employee relations, environment, human rights, product quality and safety.

Using a sample of 16,997 firm-year observations from 2003 to 2016, our main results suggest that firms with higher covenant intensity are associated with significantly lower CSR performance. Specifically, a one standard deviation increase in covenant intensity results in a 0.190 decrease in CSR strength score, which is about 12% of the average CSR score in our sample. A battery of sensitivity tests confirm that our results are robust to using alternative measures of CSR, using individual components of covenant intensity measure, and variations of econometric models.

The negative association between covenant intensity and CSR performance does not itself warrant a causal effect of covenants on CSR. It is plausible that covenant intensity and firms' CSR performance are simultaneously determined by some unobservable heterogeneities (omitted variable bias), or that firms with high CSR performance may have lower cost of debt and enjoy better loan terms with fewer covenants (reverse causality bias). We explicitly describe these potential endogeneity issues that we can think of and design specific tests to deal with them. First, to deal with the omitted variable bias, we add several other control variables that could be correlated with covenants and CSR at the same time (including media coverage, sentiment, litigation risk, and the percentage of shares owned by socially responsible institutional investors) and estimate the effect of covenant intensity on CSR with other high dimensional fixed effects. Our main results still hold.

Our second approach is to exploit the supply side determinants of loan covenants. Murfin (2012) found that banks would issue tighter covenant after suffering payment defaults to their

loan portfolios. This supply side determinant of loan covenants is largely unrelated to firm's characteristic, providing us an exogenous variation in the loan covenants. Therefore, we instrument the loan covenant intensity by the number of defaults suffered by the banks. We find that recent default experience leads to tighter loan covenant and the instrumented loan covenant leads to lower CSR performance. Thirdly, we utilize the SFAS 160 accounting rule change in 2008 on firms' reporting the noncontrolling interest in the equity section, which creates exogeneous additional covenant slack for the firms. Before the rule change, noncontrolling interest is reported either in the liabilities or in the mezzanine section between liabilities and equity. After SFAS 160 came into effect at the end of 2008, firms must report the noncontrolling interest in the equity section. This change has direct effect on firms' key financial ratios but does not affect firms' fundamentals. Following Cohen et al. (2019), we estimate the effect of this exogeneous widening distance between firms' financial ratios and the corresponding ratios in the loan covenant due to SFAS 160 on CSR performance, and find that relaxing the firms' covenant intensity leads to better CSR ratings. Fourthly, we exploit the staggered adoption of anti-recharacterization laws to examine the creditor rights protection on firm's CSR performance. In the late 1990s and early 2000s, several states adopted anti-recharacterization statutes, accelerating the process for creditors to repossess collaterals, which creates an exogenous increase in the protection of creditor rights. With the increase in creditor rights protection, firm's CSR performance improves.

We then explore the cross-firm heterogeneity of our results along two theoretically motivated dimensions to better understand the channels through which covenants affect firms' CSR ratings. We first study from the creditor's perspective, specifically how the negative effect of covenant intensity on CSR varies depending on bank characteristics. Larger and powerful banks tend to have more bargaining power in loan contracting. We expect the negative effect of covenant intensity on CSR performance to be stronger where the lenders are larger. When

the loans outstanding are shorter in maturity, the creditor-shareholder conflict is more severe, and we expect the effect of covenant intensity is stronger as well. The interaction analysis results confirm such expectations.

Second, we examine the effect of covenant intensity on CSR conditional on varied levels of conflict of interests between creditors and other stakeholders. As CSR investments are on the well-being of other stakeholders but at the cost of creditors and covenants are designed to reduce such conflicts, we expect the negative effect of covenant intensity on CSR to be more pronounced when the creditor-stakeholder conflict is stronger. Previous literature document that firms' external political environment affects firms' CSR engagement (e.g., Di Guili and Kostovetsky, 2014). Therefore, we expect that the effect of covenant intensity to be stronger for firms located in Democratic states compared to those in Republican states. We also look at socially responsible institutional investors (SRIO) who prefer high CSR performance firms and expect the effect of covenants to be stronger for those firms held more by SRIO. We further consider the conflict of interest between banks and bond holders and expect the effect to be stronger for firms with higher proportion of public debt. The empirical results confirm all these hypotheses.

It is possible that our main results of the negative effect of covenant intensity on CSR could be due to financial constraints or earnings pressure, that firms are less likely to invest in CSR. We test these two alternative stories by using multiple measures of financial constraints and earnings pressure and find inconsistent results with these alternative explanations. These results suggest that the lower CSR performance induced by covenants is not driven by financial constraints or managers trying to meet or beat the short-term earnings targets.

We have so far documented the negative effects of covenant intensity on CSR performance. An underlying assumption is that ex-post banks will intervene and reduce firms' investment in CSR, and thereby the ex-ante concerns about the drastic consequences of technical default

would result in less incentives in long-term engagement in CSR activities. This ex-ante discouragement is effective only if the ex-post consequences are binding. We verify this by directly examining what would happen to CSR investment if technical defaults really occur. Following Roberts and Sufi (2009) and Nini, Smith, and Sufi (2012), we explore the ex-post effect of covenant violations on firms' CSR performance and find CSR ratings indeed decline after covenant violations.

With recent increasing popularity of cov-lite loans in the leveraged loan market, we also explore the effect of cov-lite loan initiation on firm's CSR performance. Compared to traditional bank loan covenants, cov-lite loans only include event-based covenants. The initiation of cov-lite loans provides firms with lower level of covenant intensity (e.g., Becker and Ivashina, 2016). We provide evidence that the causes of cov-lite loans are likely orthogonal to firms' CSR performance, suggesting that cov-lite loan initiation creates an exogenous shock to covenant intensity. Using a difference-in-difference method, we estimate the treatment effect of cov-lite loan initiation on firms' CSR and find that firms' CSR performance significantly improves after the cov-lite loan initiation. This is consistent with the view that event-based covenants are less restrictive than ratio-based covenants.

Our main analysis is focused on the contract terms in loan agreements. In further explorations, we check the effect of credit default swap (CDS) trading initiation on firm's social performance. With the CDS trading initiation, banks could transfer the creditors' risks to CDS sellers. Therefore, banks would reduce their monitoring since they are less concerned about firms' repayment capabilities (Shan, Tang, Winton, 2019). Following the CDS trading initiation, creditors would intervene less about firm's investment in CSR. We find that the empirical results are consistent with our expectations that better protection of creditor rights increases firms' CSR performance, which lends further support to our creditor-shareholder conflict story.

Finally, we verify that our results are robust to alternative measures of CSR performance

and explore the real social effect of covenants on environmental and social scores. We first look at the sub-indices of CSR strength in KLD data and find that the effect of covenant intensity is strong in different dimensions of CSR, such as product, diversity, employee, environment, and community. We further find that our main results are robust to using different data sources to measure CSR, including Sustainalytics and Thomson Reuters. These results further complement our main findings of the negative effect of covenants on firms' CSR performance.

This paper contributes to the following strands of literature. First, it relates to research on the role of creditor-shareholder conflict (e.g., Jensen and Meckling, 1976; Myers, 1977). The real social impact of the conflict between creditors and shareholders is understudied due to the difficulty of empirically measuring the creditor-shareholder conflict. The majority of the extant studies rely on stock and bond price reactions to specific corporate events to infer the conflict between shareholders and bondholders (e.g., Warga and Welch, 1993; Billett, King, and Mauer, 2004). Gilje (2016) measures the creditor-shareholder conflict with changes in financial distress driven by exogenous changes in commodity prices, and study the effect on firms' risk taking. Recently, Chu (2018) and Jiang, Li, and Shao (2010) use the existence of dual holders to measure the creditor-shareholder conflicts. We contribute by studying loan covenants, the device to align shareholders' interest to that of creditors, and its social impacts on CSR. We find robust evidence that covenant intensity significantly reduces firms' CSR performance.

Second, our paper is related to the growing literature on the determinants of CSR performance. The existing literature has identified various factors affecting firms' CSR activities, such as legal origin (e.g., Liang and Renneboog, 2017), institutional ownership (e.g., Dyck et al, 2019; Chen, Dong, and Lin, 2020), and foreign competition (e.g., Flammer, 2015). There is an emerging literature recently that studies the long-horizon incentives including executives' long-term incentives and the presence of long-term institutional investors induce firms to invest more in CSR activities (e.g., Flammer and Bansal, 2017; Nguyen et al., 2020).

We extend and compliment this line of explorations by studying the effect of the misalignment of interests between creditors, and shareholders and other stakeholders as evidenced by variations in covenant tightness on firms' CSR investment decisions.

The rest of the paper is organized as follows. Section 2 describes the data, variable construction, and summary statistics. Section 3 shows the main empirical analysis of the effect of covenant intensity on CSR, including baseline results, robustness tests and endogeneity tests. Further analysis is reported in Section 4. Section 5 concludes.

2. Data, Sample, and Variables

2.1. Data and sample

Various databases are used to form our main sample. We obtain the CSR data from the Kinder, Lydenberg, and Domini Research & Analytics (KLD) database, which had been widely used in the literature (see, e.g., Deng, Kang and Low, 2013; Chen, Hong, and Lin, 2020). Data on loan covenants are extracted from the Loan Pricing Corporation's (LPC) DealScan database. We obtain firms' financial information from Compustat, stock market performance and stock liquidity from Center for Research in Security Prices (CRSP), analyst coverage from Institutional Brokers Estimates Systems (I/B/E/S), and institutional ownership from Thomson Reuter Institutional Holdings.

Our sample includes firm-year observations jointly covered by KLD, DealScan, Compustat, and CRSP. KLD increases its coverage drastically in 2003 by including the full coverage of firms in the Russell 3000 index, where the number of covered firms rises from around 1000 to around 3000 in 2003. Therefore, we start our sample from 2003 when the KLD coverage becomes stable to maintain a more balanced firm-year panel.²¹ The sample ends in 2016 because the KLD data is only available till then. We exclude firms in the financial industry

²¹ Results are qualitatively similar if we include observations from 1991.

and regulated utility industry where the SIC codes are from 4900 to 4999 and from 6000 to 6999. We also exclude firm-year observations with missing values for variables employed in the main regression. Finally, our main sample consists of 2524 firms (16,997 firm-years) between 2003 and 2016.

2.2. Variables construction

To construct our main independent variable of interest (i.e., covenant intensity), we utilize the loan-level data from DealScan, which covers 220,732 packages and 317,880 facilities with facility start year from 1981 to 2016. The loan-level data are matched to firms in Compustat using DealScan-Compustat link file (Chava and Roberts, 2008). For each firm-year observation, we consider the covenants of existing loans where the presence of a loan in a year is based on the start date and end date of the facility. Following Bharath et al. (2011) and Bradley and Roberts (2015), we construct the covenant intensity measure by adding up indicator variables concerning six dimensions in debt agreements: *MoreThan2*, *Secured*, *AssetSweep*, *DebtSweep*, *EquitySweep*, and *DividendRestriction*. *Morethan2* equals 1 if there are more than 2 covenants stipulating the financial ratios (e.g., Max. Leverage Ratio, Min. Current Ratio, etc.) in any existing loan of the firm.²² *Secured* equals 1 if any existing loan is protected by a collateral asset that is at least of the same value as the face value of the loan. *AssetSweep* equals 1 if the firm has an existing loan that requires that the firm must pay down the loan by certain proportion of the asset when it sells its asset. *DebtSweep* equals 1 if any existing loan requires that the firm must pay down certain proportion of the loan when the covenant is violated. *EquitySweep* equals 1 if any existing loan requires that the firm must pay down the loan by certain proportion of net proceeds received from the equity issuance. *DividendRestriction* equals 1 if any existing loan requires that the firm be restricted from paying dividends to its shareholders. Therefore, our covenant intensity (*CovInt*) ranges from 0 to 6.

²² The median number of financial ratio-based covenants in a loan is 2, thus we use this cutoff.

Following previous studies (e.g., Flammer, 2015; Flammer and Bansal, 2017), we measure firm's CSR performance using the strength score from KLD for the following 6 dimensions: community activities, diversity, employee relations, environment, human rights, product quality and safety. In each dimension, there are several indicator variables measuring different strength in this dimension. For example, in terms of community activities, KLD codes firm's charitable giving, innovative giving, support for housing, etc. The strength score is calculated by adding up all strength indicators in the above 6 dimensions.²³ Higher CSR rating (i.e., strength score) reflects better social performance.

We include the control variables that have been shown to affect firms' CSR performance following prior literature on the determinants of CSR performance (e.g., Brammer and Milliongton, 2005; Di Giuli and Kostovetsky, 2014; Ferrell, Liang, and Renneboog, 2016; Liang and Renneboog, 2017). Since larger firms tend to achieve better CSR performance, we control the firm size using total asset (*Size*). To control the effect of "doing good by doing well" proposed by Hong, Kubik, and Scheinkman (2012) and Xu and Kim (2020), we include firm's operating performance (*ROA*) which is the EBITA divided by book value of assets. We also include the market-to-book ratio (*MB*), defined as market value of equity over book value of equity, to account for firm's growth opportunity and include leverage ratio (*Lev*) and the cash-to-assets ratio (*Cash*) to control for the effects of capital structure and cash holdings on CSR performance. Capital investment (*CapEx*) calculated as the ratio of capital expenditure over total assets, and R&D expenditure (*RDExp*), calculated as the ratio of R&D expenses over total sales, are included to capture firm's investment intensity. Following Hirshleifer, Low, and Teoh (2012), firm-year observations with missing R&D information are assigned a zero R&D value and we include an indicator variable to denote such observations (*RDD*). To account for firm's

²³ KLD also provides the strength score on corporate governance. As rating on corporate governance is deviated from our research question on firm's social performance, we exclude this dimension in calculating the CSR rating. In untabulated results, we calculate the CSR rating by including all 7 dimensions and find similar results.

information environment, we include analyst coverage (*Analyst*), calculated as the natural logarithm of one plus the number of analysts following the firm.²⁴ We also control stock return (*Return*) defined as annual compound of monthly returns in the fiscal year. To capture the effect of stock liquidity on CSR performance (Chang et al. 2019), we include the firm's liquidity (*LIQ*), which is the opposite value of Amihud (2002)'s illiquidity. Lastly, we control the effect of institutional shareholder on CSR performance (Chen, Dong, and Lin, 2020) by including the institutional ownership (*IO*).

2.3. Descriptive Statistics

Panel A of Table 1 presents the summary statistics of variables used in our baseline analysis. *CSRStr* has a mean of 1.577 with a standard deviation of 2.578. All variables except *CSRStr* and *CSRCon* are lagged one year. *CovInt* (i.e., covenant intensity) has a mean of 2.421 with a standard deviation of 2.044. This indicates that, on average, our sample firms have covenants in more than 2 dimensions. The use of covenants is common as the 25th percentile of *CovInt* is 1, indicating that more than 75% of our firm-year observations are constrained by loan covenants. The median total asset in our sample is 1739.8 while median total asset in the Compustat-CRSP matched sample over the same period is only 285.2, suggesting that our analysis is based on larger firms. It is not surprising given that KLD and DealScan cover larger firms.

[Insert Table 1 about here]

Panel B of Table 1 displays the correlation matrix of the main variables. In particular, this univariate coefficient between *CovInt* and *CSR* is negative, supporting our prediction. Meanwhile, *CovInt* is also correlated with many firm characteristics such as firm size, leverage, etc. Smaller and heavily leveraged firms are associated with higher covenant intensity and

²⁴ To be counted as an analyst who follows the firm, he or she should have issued at least one earnings forecast for the firm in the fiscal year.

lower CSR performance. Hence, rigorous multivariate analysis is essential to examine the effect of covenant intensity on firm's CSR performance.

3. Main Findings

3.1. Baseline Results

We first examine the effect of covenant intensity on firm's CSR rating by estimating the following regression model:

$$CSR_{i,t} = \alpha + \beta CovInt_{i,t-1} + \gamma Controls_{i,t-1} + \delta Industry_i + \theta Year_t + \varepsilon_{i,t}, \quad (1)$$

where $CSR_{i,t}$ is the CSR strength (concern) rating of firm i in year t , $CovInt_{i,t-1}$ indicates firm's covenant intensity of its existing loans, β captures the average effect of covenant intensity on CSR rating, $Controls_{i,t-1}$ denotes a vector of the firm characteristics controls including size, leverage, market to book ratio, cash holdings, profitability, capital investment, R&D investment, stock market return, analyst coverage, institutional ownership, and stock liquidity. Industry fixed effects are included to account for the time-invariant omitted industry characteristics and year fixed effects are included to account for the time trends.

We show the results in Table 2. In column (1), the coefficient of $CovInt$ is negative and significant at the 1% level, indicating that firms with higher covenant intensity are associated with lower CSR performance. In terms of economic magnitude, a one standard deviation increase in $CovInt$ (2.044) is associated with a 0.190 ($2.044 * (-0.093)$) decrease in CSR strength score, which is about 12% of the average CSR score. In other words, the effect of covenant on CSR performance is not only statistically significant but also economically significant. Moreover, the coefficient of $CovInt$ in column (2) is insignificant, suggesting that higher covenant intensity does not lead to more CSR concerns. This is consistent with the view that CSR concern leads to pollution and regulatory compliance violation (Chatterji, Levine, and Toffel, 2008). Increasing the probability of regulatory compliance violation would increase

firm's downside risk, which is inconsistent with creditor's interests.

In line with the findings of Di Giuli and Kostovetsky (2014), Hong, Kubik, and Scheinkman (2012), Liang and Renneboog (2017), Chang et al. (2019), and Xu and Kim (2020), we find CSR is positively associated with size, market to book ratio, cash holdings, and ROA, and is negatively associated with leverage and liquidity.

[Insert Table 2 about here]

Our results in Table 2 reflect the role of creditors on firm CSR performance. Consistent with the agency theory of covenant proposed by Bradley and Roberts (2015), in setting up loan agreements, creditors put up covenants to align the interests between equity holders and debt holders. These covenants restrict firm's investment in CSR.

3.2. Robustness Tests

In this subsection, we perform a battery of robustness checks to validate the results in the baseline analysis. We first examine the effect of *CovInt* on alternative CSR performance measures using CSR net score and adjusted net scores. Next, we explore the effects of individual components of *CovInt* on CSR performance. Lastly, we conduct several additional sensitivity tests.

Panel A of Table 3 presents results on alternative CSR measures: net score and adjusted net score. KLD measures firm's CSR concern in the same six dimensions as for strength. Net score (*CSRNet*) is computed as the strength score minus the concern score. Adjusted net score (*CSRNetAdj*) is the net score scaled by the aggregated items using the range of scores for each year. In columns (1) and (2), we use the *CSRNet* and *CSRNetAdj* as the dependent variable and find that the coefficients of *CovInt* in both columns are negative and significant at the 1% level, indicating that our baseline results still hold when we measure CSR performance using CSR net scores.

In Panel B, we show the results using each individual component of *CovInt* as independent

variable. In all columns but column (5), the coefficients on the individual covenant intensity measure is negative and significant at the 1% level.²⁵ In other words, almost every dimension of covenant intensity is negatively associated with CSR performance, suggesting that the negative covenant intensity-CSR relationship is not driven by a particular type of covenant restriction.

Panel C presents the result from additional sensitivity tests. Our baseline results stay the same using the following alternative specifications: (a) Tobit model to take care of the bounded nature of CSR strength rating; (b) Negative binomial model to mitigate the concern that CSR strength rating is discrete rather than a continuous variable; (c) Fama-Macbeth regression to account for cross-sectional correlation among standard errors; (d) Exclude firm-year observations with zero CSR strength score to mitigate the concern that results are driven from firms which do not invest in CSR; (e) Firm fixed effects regression to account for the effect of unobserved time-invariant firm characteristics on the relationship between covenant intensity and CSR performance.

Despite the set of control variables in our baseline analysis, there may still be omitted variables affecting covenant intensity and CSR performance simultaneously. To alleviate such concern, we control for several additional variables that may drive the covenant-CSR relationship and include high-dimensional fixed effects to control for industry-year trends and state characteristics. In the last column of Panel C, we include $State \times Year$ and $Industry \times Year$ fixed effects to control for the local economic condition and industry trend. We also add several other variables that might affect the CSR performance. *MediaCoverage* is the number of news articles reported by media from RavenPack. *Sentiment* is the average event sentiment score for all the articles reported by media from RavenPack. *Gov* is the KLD CSR strength score in the

²⁵ Part of the reason that we observe insignificant results on *EquitySweep* is that equity sweep is the least commonly used covenant (23.3%) compared to asset sweep (34.3%), debt sweep (28.0%), collateral requirements (58.8%), and dividend restrictions (70.8%).

corporate governance dimension. *Litigation* is an indicator variable indicating whether the firm is in a litigious industry. *SRIO* is the percent of shares owned by socially responsible institutional investors. We follow Hwang, Titman and Wang (2018) in calculating the ownership of socially responsible institutional investors. To control for firm's financing condition, we include the S&P rating. We code the firm's S&P rating into a discrete variable goes from 1 to 7.

The coefficients of *CovInt* in all columns of Panel C are negative and significant at least at 5% level. Taken together, our main results are robust to a battery of tests using alternative CSR measures, individual components of covenant intensity, and variations of econometric models.

[Insert Table 3 about here]

3.3. Endogeneity Tests

Although we have documented a robust negative relationship between covenant intensity and CSR performance, this association is not immune from endogeneity issues. Moreover, firms with high CSR performance may have lower cost of debt and at the same time enjoy better loan agreements with fewer covenants (e.g., Shi and Sun, 2015, Gao, Li and Ma, 2020). In other words, our results may suffer from reverse causality issue. To strengthen the causal link between loan covenant and CSR, we use the supply side determinants of loan covenants and one quasi-natural experiment - the SFAS 160 accounting rule change. Moreover, to examine the creditor rights protection on firm's social performance, we examine the effect of staggered adoption of anti-recharacterization laws.

Though loan covenants are related to firm's characteristics, other factors may also determine the loan contract terms. For example, Murfin (2012) lender specific shocks impact the loan contracting. Banks would issue tighter covenants when they suffer payment defaults to their own loan portfolios. The reason behind it is that banks would update their screening ability with recent defaults, thereby impacting its contracting behavior. This supply side

determinants are largely unrelated to the firms' characteristics. Therefore, it provides us an exogenous variation in firms' loan covenants from the supply side.²⁶

Specifically, we instrument the loan covenant intensity by the number of defaults in the last half year experienced by the creditors of the existing lending contracts. For each contract, we count the number of defaults suffered by the creditor in the last half year leading up to the loan negotiation. We assume the term of loan is negotiated two months before the legal effective date of the loan. Since we are interested in economically significant defaults that may impact the behavior of the bank, we focus on borrowers' default and selective defaults covered in Standard & Poor's rating database in Compustat. To control for the lender fixed effect, we demean the number of defaults by lender, subtracting of the lender' average default count in the sample. Since there maybe multiple lenders in each loan package, we focus on the default experience of lead arrangers. For firm-year observation with multiple loan contracts, we count the average number of defaults suffered by the lead arranger 6 months prior to each loan negotiation.²⁷

Results are reported in Panel A of Table 4. In column (1), the coefficient on #Defaults is 0.051 and significant at the 1% level, suggesting that banks with recent defaults indeed issue tighter covenants. In column (2), the coefficient on the instrumented *CovInt* is -0.889 and significant at the 1% level, suggesting a causal relationship between loan covenant intensity and CSR performance.

In December 2007, to harmonize the accounting standard with the International Accounting

²⁶ Another question is why firms would accept tighter covenants. This has much to do with the frictions in the bank loan market. If there is no friction, lenders who haven't had any recent defaults would step in to provide a better contract to the borrower. Bank-borrower relationships, however, are very sticky. Borrowers are always best served by a small, close-knit circle of relationship banks rather than a perfectly competitive mass of investors in practice. Smaller bank groups, according to Peterson and Rajan (1994, 1995), allow lenders to collect rents from future business, enabling upfront borrower-specific investments needed to resolve information asymmetries. Empirically, attempts to increase the breadth of lender relationships increase the price and reduce the availability of credit (Peterson and Rajan (1994, 1995), Cole (1998)).

²⁷ Alternatively, we weight the lead arranger's number of defaults by its share of loans in each firm year. Results are qualitatively similar.

Standard Board (IASB), the Financial Accounting Standards Board issued SFAS 160 to modify the treatment of noncontrolling interest (i.e., minority interest) in a consolidated entity (Cohen et al., 2019). Before SFAS 160, noncontrolling interest is reported either in the liabilities or in the mezzanine section between liabilities and equity. After SFAS 160 came into effect in December 2008, firms must report the noncontrolling interest in the equity section. This change has direct effect on key financial ratios but no effect on firm's fundamentals. In particular, reporting the noncontrolling interest in the equity section provides additional covenant slack as the distance between firm's financial ratios and corresponding financial ratios in the loan covenants is widened. Following Cohen et al. (2019), we only consider equity-based covenants since the effect of SFAS 160 on these covenants is clear. After including the noncontrolling interests, firm's equity increases. Therefore, firm's debt to equity ratio, debt to tangible net worth ratio, and leverage ratio decreases. At the same time, firm's net worth, and tangible net worth increases.

We restrict the sample period from 2007 to 2009 to examine the effect of SFAS 160 on firm's CSR performance. The regression specification is as follows:

$$CSR_{i,t} = \alpha + \beta_1 Intense \times Post \times MIB + \beta_2 Intense + \beta_3 MIB + \beta_4 Post \times MIB + \beta_5 Intense \times Post + \beta_6 Intense \times MIB + \gamma Controls_{i,t-1} + \delta Industry_i + \theta Year_t + \varepsilon_{i,t}, \quad (2)$$

where *MIB* is an indicator variable indicating whether the firm has minority interest at the end of 2008, *Intense* is an indicator variable indicating whether the firm is close to the covenants threshold, *Post* is an indicator variable which equals 1 in year 2009, and *Controls* denote the control variables used in the baseline analysis.²⁸ β_1 captures the relative effect of SFAS 160 on firms which are close to the covenant thresholds versus those far from the covenant thresholds. Results are reported in Panel B of Table 4. The coefficient on the triple interaction term is 0.216

²⁸ We define *Intense* to be one if it is within 50% of the firm's covenant threshold. For robustness, we define firms within 30% of the covenant threshold as *Intense* and continue to find similar results.

and significant at 5% level, suggesting that relaxing the firm's covenant intensity leads to better CSR performance.

After establishing the causal link between loan covenant and firm's CSR performance. We are interested in the protection of the creditor rights matters in the firm's CSR investment. When creditor rights are strengthened or can be better protected, they are less concerned about firms' repayment capacity and would lower their impact on firm's CSR investment. To test this conjecture, we leverage the staggered adoption of anti-recharacterization statutes to examine the effect of creditor rights protection on firms' CSR performance.

The automatic stay in Chapter 11 delays or prevents creditors from repossessing the collateral. Therefore, firms transfer the collateral to special purpose vehicles (SPVs) and use the SPV as the borrowing entity to avoid the automatic stay and to facilitate borrowing. However, the transfer of collateral to SPV maybe classified as a financing transaction rather than a true sale, which is called recharacterization. The recharacterization leads to a delay for creditors to repossess the collateral. In the late 1990s and early 2000s, several states adopted anti-recharacterization statutes, which classify the transfer of collateral to SPV as true sale if it is labelled as a true sale. In short, the anti-recharacterization law helps creditors to accelerate the process to repossess collaterals, creating an exogenous increase in creditor rights protection (Janger, 2003; Li, Whited, and Wu, 2016; Chu, 2020).

As different states adopt the anti-recharacterization statutes in different year, we employ the difference-in-different specification to examine the effect of anti-recharacterization statutes on firm CSR performance.²⁹ The anti-recharacterization statutes is applied to bankruptcy firms rather than lenders (e.g., banks). Therefore, we consider the borrowing firm's headquarter state's anti-recharacterization statutes. We construct an indicator variable *ARState* that equals

²⁹ Louisiana and Tax adopted the anti-recharacterization statutes in 1997, followed by Alabama in 2001, Delaware in 2002, South Dakota in 2003, Virginia in 2004, and Nevada in 2005.

1 if the firm's headquarter state adopted the anti-recharacterization statutes and an indicator variable *Post* that equals 1 if the firm's headquarter state has adopted the anti-recharacterization statutes. We include these two dummy variables and their interaction in the regression. Given that $ARState \times Post$ is colinear with *Post*, we drop *Post* when reporting the results. Results are reported in Panel C of Table 4. The coefficient of the interaction between *ARState* and *Post* is 0.487 and significant at the 1% level. This suggests that when the creditor rights are better protected, firms' CSR performance improves.

[Insert Table 4 about here]

Taken together, we strengthen the causal relationship between covenant intensity and CSR performance by using an instrumental variable approach and quasi-natural experiment from SFAS 160 accounting rule change. We use the supply side determinants of loan covenants to show that creditors' recent default experience impacts the loan contracting and further restrict firm's CSR investment. With the covenant relaxation after the SFAS 160 accounting rule change, firm's CSR performance improves. Moreover, we also explore the effect of creditor rights protection on firm's CSR performance. We find that when creditor rights are better protected, firm's CSR performance also improves.

4. Further Analyses

4.1. Heterogeneity Tests

Previous section documents a robust negative causal impact of covenant intensity on firm's CSR performance. To shed light on the channels through which covenants affects CSR, we explore the cross-sectional heterogeneity along creditors and loans attributes, and firm characteristics.

4.1.1. Creditor Perspective

We first examine the creditor perspective, especially how bank characteristics affect the

negative relationship between covenants and CSR. Large and powerful banks tend to have stronger bargaining power in the loan contracts, and thus they tend to impose a greater pressure on borrowing firms compared to small banks. We expect the negative effect of covenant intensity on firms' CSR performance to be stronger when firms borrow from larger and more powerful banks. In addition to bank power, loan maturity also matters. CSR investments reduce firms' short-term profits for the long-run benefits for stakeholders, however, such benefits may not be enjoyed by banks who are fixed claimants with a limited horizon, especially when the loan matures in a short period of time. Hence, the creditor-shareholder conflict becomes more severe when more loans are approaching maturity, and we expect the effect of covenant intensity to be more pronounced for firms with more shorter-term loans.

To test the above arguments, we interact covenant intensity (*CovInt*) with bank power and loan maturity proxies. We construct three variables to capture bank power, namely, existence of top 4 large U.S. banks (i.e., JPMorgan Chase, Bank of America, Wells Fargo, and Citigroup) in loan contracts, proportion of loans held by top 4 banks, and the loan amount weighted average bank size by the firm. In the U.S. banking market, the top 4 banks hold 45% of all U.S. customer deposits in 2018.³⁰ We obtain banks' characteristics from BvD BankScope and match them with lenders in DealScan by lender name based on the DealScan-Lender link file from Schwert (2018). To capture bank power faced by borrowing firms, we construct an indicator variable *Top4Loan* that equals 1 if any existing loan comes from the top 4 banks for the firm, the percentage of loans that are extended by the top 4 banks (*%Top4Loan*) given that larger proportion of loans from big banks would strengthen the effect of covenant intensity, and the natural logarithm of value weighted bank size from the firm's loan portfolio. In calculating the value weighted bank size, we first calculate the size of existing loans held by each bank in each firm year. Then we calculate the average bank size by weighting each lender's size by its share

³⁰ [https://en.wikipedia.org/wiki/Big_Four_\(banking\)#United_States](https://en.wikipedia.org/wiki/Big_Four_(banking)#United_States)

of existing loans. Lastly, we calculate the proportion of loans that are due within 3 years to proxy for the loan horizon faced by firms.³¹

Results are reported in Panel A of Table 5. In column (1), the coefficient on the interaction between *CovInt* and *Top4Loan* is -0.140 and significant at the 1% level. This suggests that the effect of covenant intensity on firm's CSR performance is stronger when the big 4 banks are among the lenders. The results in column (2) are similar when we replace the *Top4Loan* dummy by the proportion of loans from top 4 banks (*%Top4Loan*). In column (3), the coefficient on the interaction between *CovInt* and *BankSize* is -0.110 and significant at the 1% level, which is also in line with our expectation that covenants set by banks with larger size would have stronger effect in discouraging firms from investing in CSR. The coefficient of the interaction between *CovInt* and *%ShortTermLoan* in column (4) is negative and significant at the 1% level, indicating that the negative effect of covenant intensity is stronger when higher proportion of loans are due in a short time period, which is inconsistent with the long-term nature of CSR investments.

Taken together, we find consistent evidence that banks play a role in affecting firm's CSR by showing stronger negative effect of covenant intensity on CSR performance when the lenders are larger and when the loans outstanding are shorter in maturity, where shorter maturity leads to more severe creditor-shareholder conflict.

4.1.2. *Conflicts of Interest*

In this section, we explore the effect of covenant intensity on firm's CSR performance when there are larger conflicts of interests between creditors and other stakeholders who may demand CSR. CSR investments provide long-run benefits mainly for stakeholders but reduces cash flows and thus repayment capabilities for bank loans, where covenants are designed to reduce such conflicts (Bradley and Roberts, 2015). Hence, we expect the negative effect of covenant

³¹ The results are similar if we consider the loans due within 1 or 2 years as short-term loans.

intensity on CSR to be stronger when the creditor-stakeholder conflicts are larger.

Di Guili and Kostovetsky (2014) document that firm's external political environment affects firm's CSR investment and firms headquartered in Democratic states have better CSR performance compared to those in Republican states. We construct an indicator variable (*Democratic*) which equals 1 if more than half of votes in the state voted for democratic candidate in the past presidential election. Chen, Dong, and Lin (2020) find that firm's CSR performance improves after there is an exogenous increase in institutional ownership caused by Russell Index reconstitutions. We focus on a subset of institutional investors who prefer high CSR performance firms, classify them as socially responsible institutional investors, and calculate the percent of shares owned by these socially responsible institutional investors (*SRIO*). Lastly, we consider the conflicts between banks and other debt holders (e.g., bond holders). Chang, Xu, and Yang (2020) argue that CSR is perceived as a commitment to public debt holders hence higher CSR helps firm to obtain public debt financing. In other words, higher proportion of public debt would create stronger conflicts between public debt holder and banks. We therefore compute the proportion of public debt over total debt (*%PubDebt*) and interact it with covenant intensity.

Panel B of Table 5 presents the results. Consistent with Di Guili and Kostovetsky (2014), we find the coefficient of *Democratic* in column (1) is significantly positive. Moreover, the coefficient on the interaction between *CovInt* and *Democratic* is -0.074 and significant at 5% level, suggesting the effect of covenant intensity on CSR performance is stronger when there exists external demand for CSR investment from the political side. In column (2), the coefficient of *SRIO* is significantly positive and the coefficient on the interaction between *CovInt* and *SRIO* is significantly negative, indicating that covenant intensity leads to larger decreases in CSR performance for firms that are held by socially responsible institutional investors. In line with the findings from Chang, Xu, and Yang (2020), we find the coefficient

of *%PubDebt* in column (3) is significantly positive and the interaction between *CovInt* and *%PubDebt* is negative and significant at the 1% level.

[Insert Table 5 about here]

To summarize, we find the effects of covenant intensity on firm's CSR performance to be stronger when banks have stronger bargaining power (e.g., more loans from large banks), when more loans are due in a short period of time, and when the conflicts between banks and other stakeholders are larger regarding CSR investments.

4.2. Additional Analysis

In section 3, we document that covenant intensity negatively affects firm's CSR performance. Our hypothesis argues that ex-post potential consequences of technical defaults (e.g., banks take control rights and intervene) increase managers' ex-ante concerns about covenant violations and thus incentivize them to reduce CSR investments. Hence, it is important to show that such ex-post consequences are tangible to make our ex-ante disincentive of investing CSR credible. To test this, we focus on the new violation and explore its effect on firm's CSR performance following Roberts and Sufi (2009) and Nini, Smith and Sufi (2012). New violation is defined as covenant violation by a firm that has not violated covenants in the past four quarters. If banks cut firm's CSR when they intervene, we expect to see a decline in firm's CSR performance after covenant violation.

Results are reported in Panel A of Table 6. In column (1), the coefficient of *Violation* is -0.046 and significant at 5% level. This indicates that firm's CSR performance decreases after covenant violation. In column (2), to closely follow the quasi-discontinuity approach used by Nini, Smith, and Sufi (2012), we control for the cash flow to asset ratio (*CFO*), leverage ratio (*Lev*), interest payment to asset ratio (*IRT*), net worth to asset ratio (*NW*), current ratio (*CR*), and market to book ratio (*MB*). The first five control variables are generally used as thresholds in the financial covenants while the last one (*MB*) is closely related to firm's investment

activities. After including the control variables, the coefficient of *Violation* is still significant and negative, showing CSR declines following covenant violations. This further corroborates our underlying hypothesis that tighter covenants increase managers' ex ante concerns and discourage them to invest in CSR which creates short-term conflicts of interest between creditors and other stakeholders.

In the prior analysis, we compute a synthetic covenant intensity index for different kinds of covenants including financial covenants (ratio-based) and event-based covenants (incurrence-based). Financial covenants are maintenance-based, meaning that firms must be compliance with the covenants in every fiscal quarter. Any incompliance with respect to the financial ratios would lead to a covenant violation. However, event-based covenants require firm's compliance only when they take certain actions. For example, an asset sales sweep covenant requires firm to pay down certain amount of the outstanding loan only when it sells its assets. Banks would not intervene if firms do not take these actions. To examine the effects of financial covenants and event-based covenants, we compute two separate covenant intensity indexes. We count the number of financial covenants and event-based covenants separately in the existing loans for each firm and normalize these two numbers to have a mean of 0 and standard deviation of 1. We include both intensity indexes in the regression and present the results in Panel B of Table 6. The coefficient of *RatioCovInt* is -0.145 and the coefficient of *EventCovInt* is -0.112. Both are significant at the 1% level. This result suggests that both types of covenants restrict firms from investing in CSR and the effect of financial covenants is relatively stronger.

With the recent increasingly popular cov-lite loans in the leveraged loan market, we examine the effect of cov-lite loan initiation on firm's CSR performance. Cov-lite loans are a type of leveraged loans syndicated to a diverse group of institutional investors, which relaxes firm's covenants intensity by using event-based covenants rather than ratio-based maintenance

ones (e.g., Stein, 2013; Becker and Ivashina, 2016). This type of leverage loans has become popular from 2007. Investors in this loan market consists of collateralized loan obligations, mutual funds, hedge funds, as well as banks and finance companies. However, banks and finance companies only make up 15% of loan origination. There are three reasons behind the rise of cov-lite loan issuance. One is about the investor base. As the investor base in cov-lite loans are different from that in traditional loans, they forgo control rights to advance yields (Stein, 2013). Another reason is about the coordination cost (Becker and Ivashina, 2016). The implementation of the covenants needs proper coordination among investors and between investors and borrowers. In a traditional bank loan contract, the number of investors is limited. However, in the leveraged loan market, there are too many investors, which would lead to a much higher coordination cost. The third reason comes from the demand side. Managers value more on the financial stability restricted by the ratio maintenance covenants after the 2008 financial crisis. These reasons are not directly linked to firm's CSR performance.

Furthermore, unlike conventional loan covenants, cov-lite loans are event-based but not maintenance covenants (Prilmeier and Stulz, 2019). Cov-lite loans only require the firm to comply with its financial covenants when a firm sells its assets, issues additional financing, etc. Therefore, *ceteris paribus*, the initiation of cov-lite loans would provide firms with lower covenant intensity. As firms with and without cov-lite loans are not randomly assigned, we find a matched control firm for each firm initiating cov-lite loan based on firm size (*Size*), leverage ratio (*Leverage*), market to book ratio (*MB*), cash holdings (*Cash*), profitability (*ROA*), capital investment (*CapEX*), R&D investment (*R&D*), and institutional ownership (*IO*). We estimate a logit model to find matched firms. Specially, the dependent variable in the logit model is an indicator variable that equals 1 if the firm has a cov-lite loan in this year. All independent variables are lagged one year. After estimating the logit model, we calculate a propensity score of having a cov-lite loan for each firm-year observations. To avoid matching cov-lite initiation

firms with firms in a cov-lite loan contract, in the matching process, we only keep observations in the cov-lite initiation year for the treated firm. After the filtering, we match each treated firm (i.e. firms with cov-lite loan initiation) with a control firm that has the closest propensity score in the same industry and in the same year. We form a 7-year event window $[-3, 3]$ around the cov-lite loan initiation for the matched sample and estimate the effect of cov-lite loan initiation on firm's CSR performance using a difference-in-difference setting. Results are reported in Panel C of Table 6. The coefficients on the interaction term $CovLite \times Post$ is significantly positive, suggesting that firm's CSR performance improves when its covenant intensity drops due to the cov-lite loan initiation.

Next, we explore the effect of CDS trading initiation on firm's CSR performance.³² CDS contracts makes credit risk transferable. With the initiation of CDS trading, lender of the firms could transfer these risks to the CDS sellers. When the creditor rights are transferable, banks would hedge their lending business risk and become less concerned about borrowing firms' long-term investment (Chang et al. 2019; Shan, Tang, and Winton, 2019). Banks may also reduce their monitoring efforts. Thus, we would expect firms to have better CSR performance after the initiation of CDS trading.

Given that CDS trading initiation is not randomly assigned, we match firms by performing a logit regression regressing the CDS initiation dummy on *Size*, *Lev*, *MB*, *Cash*, *ROA*, *CapEx*, *Analyst*, and *Return*. We calculate the propensity score of having CDS trading initiation for each firm-year observation. To avoid matching CDS trading initiation firms with firms that have CDS trading, in the matching process, we only keep observations in the CDS trading initiation year for the treated firm. After this filtering, we match each treated firm (i.e. firms with CDS trading initiation) with a control firm with closest propensity score in the same industry and in the same year and form a 3-year event window $[-1, 1]$ around the CDS trading

³² We collect the CDS trading initiation from three data sources: Markit, CreditTrade, and the GFI Group.

initiation. We construct two dummy variables: *CDS* is a dummy variable that equals 1 if the firm has a CDS trading initiation; *Post* is a dummy variable that equals 1 after the CDS trading initiation. Using the difference-in-difference setting, we estimate the effect of CDS trading initiation on firm's CSR performance and present the results in Panel D of Table 6. The coefficients on the interaction term *CDS* and *Post* is significantly positive. This suggests that firm's CSR performance improves when lenders can hedge their lending business risk with CDS.

Taken together, the empirical results are consistent with our expectations that better protection of creditor rights improves firms' CSR performance, which further supports our creditor-shareholder conflict story.

[Insert Table 6 about here]

4.3. Different Aspects of CSR

In this section, we explore the real social effect of covenants on environmental and social aspects and further verify that our results are robust across different CSR measures from various data sources. First, we look at the sub-indices (i.e., Product, Diversity, Human, Employee, Environment, and Community) in firm's CSR strength provided by KLD and present the results in Panel A of Table 7. We find that the coefficients of *CovInt* in all columns but column (3) are significantly negative, suggesting that the effect of *CovInt* is strong in general and the negative relationship between *CovInt* and CSR performance hold in different dimensions of CSR.

In addition to the KLD data, we also extract environmental ratings and social ratings from two alternative CSR data providers, Thomson Reuters ESG database and Sustainalytics.³³ In

³³ The original ASSET4 ESG data is reconstructed by Thomson Reuters to create the new Thomson Reuters ESG data, where the ASSET4 ESG data are also widely used in literature (e.g., Cheng, Ioannou, and Serafeim, 2014; Ferrell, Liang, and Renneboog, 2016; Flammer and Kacperczyk, 2019). Starting from 2002, the data cover more than 6,000 public companies globally, covering 80% of global market capitalization in 210 countries with over more than 450 ESG data points. Thomson Reuters ESG database computes a score for each category and evaluates firms' environmental performance in three categories: Resource Use, Emission,

particular, we obtain the overall ESG score, the three category scores in the environmental dimension, four category scores in the social dimension, and one category score in the strategy dimension from Thomson Reuters ESG database, and retrieve the environmental score and the social score from Sustainalytics. We then repeat the regression in Table 2 with various CSR measures from these two data sources and present the results in Panels B and C of Table 7, respectively. We find that our results are similar as *CovInt* has a significantly negative effect on most CSR performance measures.

[Insert Table 7 about here]

4.4. Alternative Explanation

In this section, we explicitly discuss several alternative explanations for our negative covenant intensity-CSR relationship and perform empirical analysis. For example, covenant intensity may simply proxy financial constraint so firms with limited financial resources invest less in CSR investments. We should observe the effect of covenant intensity to be more pronounced for more financially constrained firms. To test this possibility, we compute three financial constraint indexes following Lamont, Polk, and Saa-Requejo (2001), Whited and Wu (2006), and Hadlock and Pierce (2010) and interact each financial constraint index with the covenant intensity separately. Results are reported in Panel A of Table 8. We find that the coefficients of the interactions between *CovInt* and the financial constraint index are positive and significant in all three columns, suggesting that covenant intensity has a stronger impact on CSR for less financially constrained firms, which is inconsistent with this alternative explanation.

Another possibility is that the negative effect of covenants may be due to earnings pressure

Innovation; and social performance in four categories: Workforce, Human Rights, Community, and Product Responsibility; and governance performance in three categories: CSR Strategy, Management, and Shareholders. Sustainalytics starts in 2009 and provides ESG ratings by measuring how well companies proactively manage environmental, social and governance issues that are the most material to their business, covering 11,000 companies with 200+ indicators. Sustainalytics employs their proprietary method to compute the environment score, social score, and total ESG score which we use as alternative CSR performance measures.

and thus managers have less incentives to invest in CSR. If this is the case, we should expect the negative effect to be stronger for firms with greater earnings pressure. We thus construct three variables to proxy the earnings pressure following the literature (e.g., Roychowdhury, 2006; Zang, 2012). The first variable *Beat* is an indicator variable that equals 1 if the firm beats analysts' consensus earnings forecast. The second one *SProfit ROA* is an indicator variable that equals 1 if the firm's ROA is just above 0 and less than 0.005. The third one *SProfit EPS* is an indicator variable that equals 1 if the firm's EPS is just above 0 and less than 0.05. We interact each variable with covenant intensity separately and present the results in Panel B of Table 8. The coefficients of the interactions between *CovInt* and the earnings pressure proxies are all statistically insignificant. These results suggest that the lower CSR performance induced by covenants is not driven by managers trying to meet or beat the short-term earnings targets.

[Insert Table 8 about here]

5. Conclusion

This paper examines the real social impact of creditor-shareholder conflict by studying how terms in loan contract agreements affect firm's CSR performance. Using a balanced sample of U.S. firms from 2003 to 2016, we find that higher covenant intensity leads to weaker CSR performance. The results hold in a battery of robustness tests and identification tests. This finding is consistent with the agency theory of covenants (Smith and Warner, 1979; Bradley and Roberts, 2015). In setting up loan agreements, creditors put up covenants to mitigate the conflicts between creditors and shareholders (Jensen and Meckling, 1976; Myers, 1977).

Regarding the cross-sectional test, we find that the effect is more pronounced when creditors have greater bargaining power, when outstanding loans are closer to maturity, and when the conflict of interest between creditors and other stakeholders is larger. Moreover, we find the effect is not concentrated in firms who are financially constrained or firms who are

facing greater earnings pressure.

In the additional analysis, we find that when creditors rights are better protected (e.g., the adoption of Anti-Recharacterization statutes) and when the lending business risks can be hedged for banks (e.g., the initiation of credit default swap trading), firm's CSR performance improves. This lends further credence to the shareholder-creditor conflict story.

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Table 1: Descriptive Statistics

This table presents the descriptive statistics of our data. This sample covers firm-year observations covered by Compustat, DealScan, and KLD from 2003 to 2016. *CSRStr* (*CSRCon*) is the CSR strength (concern) rating by summing up the strength (concern) ratings of the six dimensions in KLD database, including community, diversity, employee relations, environment, human rights, and product quality. *CovInt* is covenant intensity which is a sum of 6 dummy variables: *Secured*, *Morethan2*, *AssetSweep*, *DebtSweep*, *EquitySweep*, and *DividendRestriction*. *Secured* equals 1 if any existing loan is protected by a collateral asset that is at least of the same value as the face value of the loan. *Morethan2* equals 1 if there are more than 2 financial ratio covenants (e.g., Max. Leverage Ratio, Min. Current Ratio, etc.) in any existing loan. *AssetSweep* equals 1 if any existing loan requires that the firm must pay down the loan by certain proportion of the asset when it sells its asset. *DebtSweep* equals 1 if any existing loan requires that the firm must pay down certain proportion of the loan when the covenant is violated. *EquitySweep* equals 1 if any existing loan requires that the firm must pay down the loan by certain proportion of net proceeds received from the equity issuance. *DividendRestriction* equals 1 if any existing loan requires that the firm is restricted from paying dividends to its shareholders. Other variables: *Size* is the natural logarithm of firm's total assets. *Lev* is the total debt to total assets ratio. *MB* is the market to book equity ratio. *Cash* is the cash to assets ratio. *ROA* is the operating income (EBITA) to assets ratio. *CapEX* is the capital expenditures to assets ratio. *Analyst* is the natural logarithm one plus the number of analysts covering this firm. *RDExp* is the R&D expenses to sales ratio where the missing values are filled up by 0. *RDD* is a dummy variable that equals 1 if the firm's R&D expenditure is missing. *Return* is annual compound of monthly returns in the fiscal year. *IO* is the institutional ownership at the end of the year. *LIQ* is the opposite value of Amihud illiquidity in the fiscal year. All control variables are winsorized by 0.5% at both ends. Panel A presents the summary statistics and Panel B presents the pairwise between variables in Panel A. Significant correlation coefficients at 5% level are bolded.

Panel A: Summary Statistics

Variables	Observations	Mean	S.D.	Q1	Median	Q3
<i>CSRStr</i>	16997	1.577	2.578	0	1	2
<i>CSRCon</i>	16997	1.467	1.677	0	1	2
<i>CovInt</i>	16997	2.421	2.044	1	2	4
<i>Size</i>	16997	7.525	1.561	6.426	7.391	8.501
<i>Lev</i>	16997	0.244	0.175	0.111	0.23	0.351
<i>MB</i>	16997	3.669	7.548	1.481	2.253	3.585
<i>Cash</i>	16997	0.122	0.134	0.029	0.077	0.17
<i>ROA</i>	16997	0.089	0.102	0.054	0.09	0.133
<i>CapEX</i>	16997	0.057	0.067	0.02	0.036	0.067
<i>Analyst</i>	16997	2.141	0.703	1.642	2.179	2.691
<i>RDExp</i>	16997	0.061	0.715	0	0	0.024
<i>RDD</i>	16997	0.411	0.492	0	0	1
<i>Return</i>	16997	0.179	0.573	-0.133	0.107	0.368
<i>IO</i>	16997	0.746	0.241	0.64	0.798	0.909
<i>LIQ</i>	16997	-0.035	0.832	-0.007	-0.001	0

(To be continued)

Table 1: Descriptive Statistics – continued*Panel B: Correlation Matrix*

	<i>CSRStr</i>	<i>CSRCon</i>	<i>CovInt</i>	<i>Size</i>	<i>Lev</i>	<i>MB</i>	<i>Cash</i>	<i>ROA</i>	<i>CapEX</i>	<i>Analyst</i>	<i>RDExp</i>	<i>RDD</i>	<i>Return</i>	<i>IO</i>	<i>LIQ</i>
<i>CSRStr</i>	1														
<i>CSRCon</i>	0.343	1													
<i>CovInt</i>	-0.274	-0.049	1												
<i>Size</i>	0.591	0.397	-0.257	1											
<i>Lev</i>	-0.002	0.044	0.212	0.236	1										
<i>MB</i>	0.051	-0.018	-0.016	-0.006	0.189	1									
<i>Cash</i>	0.035	-0.088	-0.13	-0.22	-0.325	0.083	1								
<i>ROA</i>	0.106	0.005	-0.064	0.103	-0.127	0.125	-0.136	1							
<i>CapEX</i>	-0.074	0.016	-0.01	-0.031	0.072	-0.012	-0.145	-0.023	1						
<i>Analyst</i>	0.412	0.179	-0.229	0.626	0.022	0.056	0.021	0.154	0.092	1					
<i>RDExp</i>	-0.013	-0.027	-0.025	-0.085	-0.023	0.026	0.249	-0.29	-0.036	-0.022	1				
<i>RDD</i>	-0.15	-0.015	0.102	-0.007	0.153	-0.035	-0.2	-0.013	0.211	-0.08	-0.071	1			
<i>Return</i>	-0.046	0.005	0.04	-0.094	-0.041	0.098	0.092	0.073	-0.054	-0.13	-0.007	-0.016	1		
<i>IO</i>	-0.022	-0.043	0.032	0.084	-0.038	-0.004	-0.019	0.128	-0.078	0.195	-0.059	-0.042	-0.05	1	
<i>LIQ</i>	0.004	0.008	-0.02	0.059	-0.012	0.007	-0.004	0.027	0.014	0.064	-0.004	-0.01	-0.029	0.057	1

Table 2: Covenant Intensity on CSR Rating

This table presents results from OLS regression of covenant intensity on CSR rating. This sample covers firm-year observations from 2003 to 2016. *CSRStr* (*CSRCon*) is the strength (concern) score by adding all the strength (concern) ratings of six dimensions in KLD database, including community, diversity, employee relations, environment, human rights, and product quality. *CovInt* is lagged the covenant intensity index. All other variables are lagged one year. See variable definitions in Table 1. 2-digit Industry and year fixed effects are included. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Variables	(1) <i>CSRStr</i>	(2) <i>CSRCon</i>
<i>CovInt</i>	-0.093*** (-6.0)	0.009 (0.9)
<i>Size</i>	1.023*** (21.0)	0.547*** (16.8)
<i>Lev</i>	-1.074*** (-5.5)	-0.519*** (-3.7)
<i>MB</i>	0.017*** (4.8)	0.003 (1.6)
<i>Cash</i>	1.872*** (7.5)	0.015 (0.1)
<i>ROA</i>	0.831*** (3.3)	-0.702*** (-4.0)
<i>CapEX</i>	0.758 (1.5)	-0.104 (-0.3)
<i>Analyst</i>	0.130** (2.2)	-0.170*** (-4.3)
<i>RDExp</i>	0.011 (0.6)	-0.036** (-2.4)
<i>RDD</i>	-0.324*** (-4.2)	-0.101* (-1.9)
<i>Return</i>	-0.034 (-1.4)	0.077*** (3.9)
<i>IO</i>	-0.902*** (-7.0)	-0.452*** (-4.7)
<i>LIQ</i>	-0.095*** (-8.3)	-0.024 (-1.5)
Fixed Effects	Industry&Year	Industry&Year
Observations	16,997	16,997
Adjusted R ²	0.457	0.379

Table 3: Robustness Checks

This table presents the robustness check results. Panel A reports results with alternative CSR Measures. *CSR Net* and *CSR NetAdj* are CSR net score (strength minus concern) and adjusted CSR net scores, respectively. Panel B reports the results for 6 components of *CovInt*: *Secured*, *MoreThan2*, *AssetSweep*, *DebtSweep*, *EquitySweep*, and *DividendRestriction*. *CSRStr* is the dependent variable for all models. Panel C reports the results from additional sensitivity tests. Tobit model in column (1) is left-censored at 0 and right-censored at 63 (i.e., the maximum value of CSR). Results from Negative binomial model and Fama-MacBeth are reported in column (2) and (3), respectively. In column (4), firm-year observations with *CSR* equals 0 are dropped. In column (5), firm fixed effects, instead of industry fixed effects, are included. In column (6), industry*year fixed effects and state*year fixed effect are included. *MediaCoverage*, *Sentiment*, *Gov*, *Litigation*, *SRIO*, and *Rating* are controlled in column (6). *MediaCoverage* is the number of news articles in RavenPack divided by 100. *Sentiment* is the average event sentiment score for all the articles reported by media from RavenPack. *Gov* is the corporate governance strength score in KLD. *Litigation* is a dummy variable that equals 1 if the firm is in a litigious industry. We classify firms as in the litigious industry if its four-digit SIC code belongs to 2833-2836, 3570-3577, 3600-3674, 5200-5961, 7370. *SRIO* is the percent of shares owned by socially responsible institutional investors (Hwang, Titman, and Wang, 2018). *Rating* is the Standard & Poor rating goes from 1 to 7. Control variables in Table 2 are included in all columns. See variable definition in Table 1. Unless otherwise specified, industry and year fixed effects are included in the regression. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Alternative CSR Measures</i>		
Variables	(1) <i>CSR Net</i>	(2) <i>CSR NetAdj</i>
<i>CovInt</i>	-0.102*** (-5.8)	-0.024*** (-5.6)
Controls	YES	YES
Fixed Effect	Industry&Year	Industry&Year
Observations	16,997	16,994
Adjusted R-squared	0.258	0.254
<i>(To be continued)</i>		

Table 3: Robustness Checks - continued*Panel B: Covenant Intensity Sub-Indices*

Intensity	Dependent Variable: <i>CSRStr</i>					
	(1) <i>Secured</i>	(2) <i>MoreThan2</i>	(3) <i>AssetSweep</i>	(4) <i>DebtSweep</i>	(5) <i>EquitySweep</i>	(6) <i>DividendRestriction</i>
Intensity	-0.300*** (-4.6)	-0.247*** (-4.6)	-0.283*** (-4.8)	-0.304*** (-4.9)	-0.101 (-1.5)	-0.424*** (-5.8)
Controls	YES	YES	YES	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year	Industry&Year	Industry&Year	Industry&Year
Observations	16,997	16,997	16,997	16,997	16,997	16,997
Adjusted R ²	0.456	0.455	0.455	0.455	0.453	0.458

Panel C: Alternative Model Specifications

Model Specification	Dependent Variable: <i>CSRStr</i>					
	(1) Tobit	(2) Negative Binomial	(3) Fama-MacBeth	(4) Exclude Zero CSR Rating	(5) Firm Fixed Effects	(6) Other Fixed Effects
<i>CovInt</i>	-0.093*** (-6.1)	-0.067*** (-6.1)	-0.089*** (-5.8)	-0.075*** (-3.0)	-0.040** (-2.6)	-0.066*** (-3.7)
Controls	YES	YES	YES	YES	YES	YES
Other Controls	NO	NO	NO	NO	NO	YES
Fixed Effects	Industry&Year	Industry&Year	Industry	Industry&Year	Firm&Year	Industry×Year State×Year
Observations	16,997	16,997	16,997	8,697	16,710	9,649
(Pseudo) R ²	0.130	0.175	0.491	0.425	0.752	0.581

Table 4: Tests on Endogeneity

This table presents the endogeneity test results. Panel A presents the results examining the effects of covenant intensity on CSR performance using the supply-side determinants of loan covenants as the instrumental variable: *#Defaults*. Column (1) in Panel A presents the results on the first stage of the 2-stage least square regression. Column (2) and (3) present the effects of instrumented covenant intensity on firm's CSR strength and concern score, respectively. *#Defaults* is the average number of defaults experienced by the lead banks in the last half year when respective loans are contracted. Kleibergen-Paap Wald F-statistics is reported for the 2SLS regressions. Panel B presents the results examining the effects of SFAS 160 accounting rule change on CSR. Sample is restricted to firm-year observations between 2007 and 2009. *Post* is a dummy variable that equals 1 for years after 2008. *MIB* is a dummy variable that equals 1 if the firm's minority interest at the end of 2008 is positive. *Intense* is a dummy variable that equals 1 if the firm's distance to violation is within 50% threshold. Panel C presents the effects of anti-recharacterization law on CSR strength and concern scores. *ARState* is a dummy variable indicates that the firm's headquarter state passed the anti-recharacterization law. *Post* is a dummy variable that equals 1 if the firm's headquarter state has passed the anti-recharacterization law. *ARState*×*Post* is colinear with *Post* and therefore *Post* is dropped in the regression. To avoid spurious inference induced by including observations too far away from the event, the sample period is restricted to 2003-2010 (Chu, 2020). Control variables in Table 2 and industry and year fixed effects are included in all columns. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Instrumental Variable

Variables	(1) <i>CovInt</i>	(2) <i>CSRStr</i>	(3) <i>CSRCon</i>
<i>#Defaults</i>	0.051*** (3.6)		
<i>CovInt</i>		-0.889*** (-2.8)	-0.015 (-0.1)
Controls	YES	YES	YES
Fixed Effect	Industry&Year	Industry&Year	Industry&Year
Observations	16,997	16,997	16,997
Adjusted R ²	0.237	0.063	0.212
Wald F-statistic		13.07	13.07

Panel B: SFAS 160

Variables	(1) <i>CSRStr</i>	(2) <i>CSRCon</i>
<i>Intense</i> × <i>Post</i> × <i>MIB</i>	0.216** (2.1)	-0.066 (-0.6)
<i>Intense</i>	-0.185 (-1.6)	-0.094 (-1.0)
<i>MIB</i>	-0.177 (-1.4)	0.133 (1.4)
<i>Post</i> × <i>MIB</i>	0.010 (0.2)	0.082* (1.8)
<i>Intense</i> × <i>Post</i>	-0.082** (-2.2)	0.035 (0.8)
<i>Intense</i> × <i>MIB</i>	0.060 (0.2)	-0.074 (-0.4)
Controls	YES	YES
Fixed Effect	Industry&Year	Industry&Year
Observations	5,485	5,485
Adjusted R ²	0.384	0.366

Panel C: Anti-Recharacterization Law

Variables	(1) <i>CSRStr</i>	(2) <i>CSRCon</i>
<i>ARState</i> × <i>Post</i>	0.487*** (2.6)	0.094 (0.5)
<i>ARState</i>	-0.688*** (-3.7)	0.016 (0.1)
Controls	YES	YES
Fixed Effect	Industry&Year	Industry&Year
Observations	15,178	15,178
Adjusted R-squared	0.398	0.352

Table 5: Cross-sectional Tests

This table presents results from the cross-section tests exploring the heterogeneity effects of covenant intensity. Panel A reports the results of interaction between covenant intensity and characteristics of firm's bank loan portfolio. *Top4Loan* is a dummy variable that equals 1 if the any existing loan is borrowed from 4 largest banks (i.e., Wells Fargo, Bank of America, JP Morgan Chase, and Citi Group). *%Top4Loan* counts the percentage amount of existing loans borrowed from the above 4 banks. *BankSize* is the natural logarithm of average bank (lender) asset weighted by existing loans. *%ShortTermLoan* is the percentage amount of loans due in 3 years. Panel B reports the results of interactions between covenant intensity and other stakeholders' interests. *Democratic* is a dummy variable that equals 1 if the votes for democratic party in firm's headquarter state is higher than 50% in the last presidential election. *SRIO* is percentage of shares owned by socially responsible institutional investors. *%PubDebt* is the percentage of debt comes from bonds rather than bank loans. Control variables in Table 2 and industry and year fixed effects are included in all columns. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Creditor

Variables	(1) <i>CSRStr</i>	(2) <i>CSRStr</i>	(3) <i>CSRStr</i>	4 <i>CSRStr</i>
<i>CovInt</i> × <i>Top4Loan</i>	-0.140*** (-4.5)			
<i>Top4Loan</i>	0.015 (0.1)			
<i>CovInt</i> × <i>%Top4Loan</i>		-0.028*** (-2.6)		
<i>%Top4Loan</i>		0.058 (1.1)		
<i>CovInt</i> × <i>BankSize</i>			-0.110*** (-6.7)	
<i>BankSize</i>			0.242*** (4.6)	
<i>CovInt</i> × <i>%ShortTermLoan</i>				-0.069*** (-2.66)
<i>%ShortTermLoan</i>				0.460*** (5.64)
<i>CovInt</i>	0.034 (1.2)	-0.049** (-2.5)	1.416*** (6.4)	-0.055*** (-2.87)
Controls	YES	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year	Industry&Year
Observations	14,687	14,687	14,687	16,996
Adjusted R ²	0.460	0.459	0.460	0.459

(To be continued)

Table 5: Cross-sectional Tests - continued*Panel B: Conflicts of Interest*

Variables	(1) <i>CSRStr</i>	(2) <i>CSRStr</i>	(3) <i>CSRStr</i>
<i>CovInt</i> × <i>Democratic</i>	-0.074** (-2.5)		
<i>Democratic</i>	0.392*** (3.3)		
<i>CovInt</i> × <i>SRIO</i>		-0.501*** (-4.4)	
<i>SRIO</i>		4.683*** (10.1)	
<i>CovInt</i> × <i>%PubDebt</i>			-0.218*** (-2.6)
<i>%PubDebt</i>			0.666*** (2.9)
<i>CovInt</i>	-0.045** (-2.2)	0.061** (2.2)	-0.071*** (-4.4)
Controls	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year
Observations	16,281	16,997	16,997
Adjusted R ²	0.459	0.472	0.459

Table 6: Additional Analysis

This table presents results from further analysis of creditors on CSR performance. Panel A presents the results of covenant violation on CSR rating. Dependent variable $\Delta CSRStr(t+1)$ is the CSR strength rating in year t+1 minus the CSR strength rating in year t. *Violation* is a dummy variable that equals 1 if the firm violates any financial covenants. *CFO* is the operating cash flow to asset ratio. *Lev* is the debt to asset ratio. *IRT* is the interest payment to asset ratio. *NW* is the net worth to asset ratio. *CR* is the current ratio (i.e., current assets over current liability). *MB* is the market equity to book equity ratio. Industry and year fixed effects are included in both columns. Panel B examines the effects of ratio-based covenant and event-based covenants simultaneously. *RatioCovInt* is the covenant intensity index counting the number of financial covenants (maintenance covenants) for each firm in each year. *EventCovInt* is the covenant intensity index counting the number of event-based covenants (e.g., asset sweep covenant) for each firm in each year. Both covenant intensity indexes are normalized to have a mean of 0 and standard deviation of 1. Panel C presents the results examining the effect of CovLite loan initiation on CSR. Logit regression is performed by regressing CovLite Loan dummy on lagged firm characteristics: *Size*, *Leverage*, *MB*, *Cash*, *ROA*, *CapEX*, *R&D* and *IO*. Propensity score for each firm-year observation is calculated based on the coefficients from the logit regression. For each firm with CovLite loan initiation, we match one control firm with the closet propensity score within the same 2-digit industry and the same year. Control firms are restricted to those without any CovLite loans. 7-year (-3, 3) window is formed for each firm around the CovLite loan initiation. *Post* is a dummy variable that equals 1 after the CovLite loan initiation. *CovLite* is a dummy variable that equals 1 if the firm has a CovLite loan initiation. Panel D presents the results examining the effect of credit default swap (CDS) trading on CSR. Logit regression is performed by regressing CDS trading initiation dummy on lagged firm characteristics: *Size*, *Lev*, *MB*, *Cash*, *ROA*, *CapEX*, *Analyst*, and *Return* with industry and year fixed effects. Propensity score for each firm-year observation is calculated based on the coefficients from the logit regression. For each firm with CDS trading initiation, we match one control firm with the closet propensity score within the same 2-digit industry and the same year. Control firms are restricted to those without any CDS trading. 3-year (-1, 1) window is formed for each firm around the CDS trading initiation. *Post* is a dummy variable that equals 1 after the CDS trading initiation. *CDS* is a dummy variable that equals 1 if the firm has a CDS trading initiation. Standard errors are clustered at the firm level. T-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Covenant Violation

Variables	(1) $\Delta CSRStr(t+1)$	(2) $\Delta CSRStr(t+1)$
<i>Violation</i>	-0.046** (-2.1)	-0.042* (-1.9)
<i>CFO</i>		0.069* (1.9)
<i>Lev</i>		-0.074* (-1.8)
<i>IRT</i>		-0.134*** (-13.9)
<i>NW</i>		-7.558*** (-4.4)
<i>CR</i>		-0.006** (-2.3)
<i>MB</i>		0.003*** (2.6)
Fixed Effects	Industry&Year	Industry&Year
Observations	13,177	11,107
Adjusted R ²	0.000	0.002

Table 6: Additional Analysis – continued

<i>Panel B: Ratio Based versus Event Based Covenants</i>	
Variables	<i>CSRStr</i>
<i>RatioCovInt</i>	-0.145*** (-4.5)
<i>EventCovInt</i>	-0.112*** (-3.1)
Controls	YES
Fixed Effects	Industry&Year
Observations	16,997
Adjusted R ²	0.459
<i>Panel C: Cov Lite Loan Initiation</i>	
Variables	<i>CSRStr</i>
<i>CovLite</i> × <i>Post</i>	0.393* (1.8)
<i>CovLite</i>	-0.561* (-1.8)
<i>Post</i>	-0.378** (-2.4)
Observations	1,674
Adjusted R ²	0.008
<i>Panel D: CDS Initiation</i>	
Variables	<i>CSRStr</i>
<i>CDS</i> × <i>Post</i>	0.174** (2.1)
<i>CDS</i>	0.365** (2.5)
<i>Post</i>	-0.078 (-1.2)
Observations	1,609
Adjusted R ²	0.014

Table 7: Different Aspects of CSR

This table presents results on the real effects of covenant intensities. Panel A presents results on the KLD CSR strength in six dimensions: Product, Diversity, Human, Employee, Environment, and Community. Panel B presents results on overall ESG score, social score, and environment scores from Sustainalytics. Panel C presents results on ESG total score and sub-indices scores from Thomson Reuters. Control variables in Table 2 and industry and year fixed effects are included in all columns. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: KLD Sub-Indices

Variables	(1) Product	(2) Diversity	(3) Human	(4) Employee	(5) Environment	(6) Community
<i>CovInt</i>	-0.007*** (-4.2)	-0.041*** (-5.2)	-0.000 (-0.1)	-0.018*** (-3.6)	-0.019*** (-3.6)	-0.011*** (-3.3)
Controls	YES	YES	YES	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year	Industry&Year	Industry&Year	Industry&Year
Observations	14,343	14,343	14,343	14,343	14,343	14,343
Adjusted R ²	0.136	0.359	0.112	0.277	0.327	0.245

Panel B: Thomson Reuters ESG Scores

Variables	(1) ESGScore	(2) ResourcesUse	(3) Emissions	(4) Innovation	(5) Workforce	(6) HumanRights	(7) Community	(8) Product Responsibility	(9) CSR Strategy
<i>CovInt</i>	-1.142*** (-5.9)	-1.574*** (-4.8)	-1.479*** (-4.5)	-1.122*** (-3.7)	-1.425*** (-4.6)	-1.269*** (-4.9)	-0.320 (-1.2)	-0.526 (-1.6)	-0.967*** (-3.1)
Observations	6,548	6,548	6,548	6,548	6,548	6,548	6,548	6,548	6,548
Adjusted R ²	0.455	0.397	0.383	0.112	0.323	0.293	0.238	0.190	0.379

(To be continued)

Table 7: Different Aspects of CSR - continued

<i>Panel C: Sustainability ESG Scores</i>			
Dependent Variables	(1) Total ESG Score	(2) Social Score	(3) Environment Score
<i>CovInt</i>	-0.434*** (-3.5)	-0.435** (-2.6)	-0.631*** (-3.3)
Controls	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year
Observations	4,284	3,975	3,975
Adjusted R ²	0.389	0.315	0.401

Table 8: Rule-out Alternatives

This table presents the results from cross-section tests ruling out alternative explanations. Panel A reports the results of interaction between covenant intensity and proxies for financial constraints. *KZ Index* is financial constraints index calculated following Lamont, Polk, and Saa-Requejo (2001). *WW Index* is calculated following Whited and Wu (2006). *HP Index* is calculated following Hadlock and Pierce (2010). Panel B reports the results of interaction between covenant intensity and earnings pressure measures. *Beat* is a dummy variable that equals 1 if the firm's earning beats the consensus forecast by the financial analysts. *Sprofit ROA* is a dummy variable that equals 1 if the firm's ROA is larger than 0 and less than 0.005. *Sproit EPS* is a dummy variable that equals 1 if the firm's EPS is larger than 0 and less than 0.05. Control variables in Table 2 and industry and year fixed effects are included in all columns. Standard errors are clustered at the firm level and t-statistics are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

<i>Panel A: Financial Constraints</i>			
Variables	(1) <i>CSRStr</i>	(2) <i>CSRStr</i>	(3) <i>CSRStr</i>
<i>CovInt</i> × <i>KZ Index</i>	0.045*** (3.8)		
<i>KZ Index</i>	-0.214*** (-4.2)		
<i>CovInt</i> × <i>WW Index</i>		2.274*** (8.5)	
<i>WW Index</i>		-6.863*** (-6.5)	
<i>CovInt</i> × <i>HP Index</i>			0.206*** (6.4)
<i>HP Index</i>			-0.442*** (-3.7)
<i>CovInt</i>	-0.115*** (-6.6)	0.720*** (8.3)	0.709*** (5.9)
Controls	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year
Observations	16,969	16,971	16,997
Adjusted R ²	0.461	0.476	0.464
<i>(To be continued)</i>			

Table 8: Rule-out Alternatives -continued*Panel B: Earnings Pressure*

Variables	(1) <i>CSRStr</i>	(2) <i>CSRStr</i>	(3) <i>CSRStr</i>
<i>CovInt</i> × <i>Beat</i>	-0.063 (-1.0)		
<i>Beat</i>	0.163 (0.8)		
<i>CovInt</i> × <i>SProfit ROA</i>		0.004 (0.1)	
<i>SProfit ROA</i>		-0.157 (-0.5)	
<i>CovInt</i> × <i>SProfit EPS</i>			0.007 (0.1)
<i>SProfit EPS</i>			-0.029 (-0.1)
<i>CovInt</i>	-0.096*** (-6.8)	-0.093*** (-6.0)	-0.093*** (-6.0)
Controls	YES	YES	YES
Fixed Effects	Industry&Year	Industry&Year	Industry&Year
Observations	11,878	16,997	16,995
Adjusted R ²	0.397	0.457	0.457

Combating the COVID-19 Pandemic: The Role of the SARS Imprint

March 17, 2021

Abstract

We provide evidence of delayed attention and inaction in response to COVID-19 in countries that did not experience SARS in 2003. Using cross-country data, we find that individuals in countries that had SARS infections in 2003 search more intensively for COVID-19-related information on Google in late January 2020, the time of the first known outbreak in Wuhan, China. Early attention to the novel virus, as measured by Google searches, is associated with deeper stock market drops in countries with SARS experience. In contrast, people in countries without SARS experience started to pay more attention much later, in March. Moreover, governments in these countries responded significantly more slowly in implementing social distancing policies to combat domestic COVID-19 outbreaks than governments in countries with SARS experience. Moreover, such early responses of individuals and governments in countries with SARS experience are prevalent within continent, even in non-Asian countries. Furthermore, people in countries with SARS experience are more compliant with social distancing rules. These timely attention and proactive responses of individuals and governments are more pronounced in countries that reported deaths caused by SARS, which left deeper imprints. Our findings suggest that the imprint of similar viruses' experience is a fundamental mechanism underlying timely responses to COVID-19.

Keywords: COVID-19, Imprint, SARS Experience, Delayed Response

JEL Classification: D83, E70, G10, H12, I10

1. Introduction

The coronavirus disease 2019 (COVID-19) is one of the deadliest global pandemics in human history, one that accounted for more than 1.8 million deaths worldwide in 2020 and is estimated to cause a 4.3% contraction in global GDP in 2020.³⁴ Despite its severity, people and governments across the globe respond differently in their containment measures and economic policies (e.g., Ding et al., 2020a, 2020b; Huang et al., 2020).

This paper studies how the imprint of an experience with similar viral outbreaks affects countries' responses to COVID-19. In particular, we explore the heterogeneous attention and responses to the first known outbreak of COVID-19 in Wuhan, China, between countries severely affected by Severe Acute Respiratory Syndrome (SARS) in 2003 and other countries, given the similarity between the viruses that cause SARS and COVID-19. We find that countries with experience of SARS infections pay attention to COVID-19 earlier and respond in a more timely and proactive manner.

First, we directly examine whether an imprint of SARS experience exists using Google search data. We study people's attention to the first known outbreak of COVID-19 in China from January 20 to January 31, 2020, which covers the initial government responses and the first extensive media coverage of COVID-19 in China and abroad. We find that searches for "SARS" and "coronavirus" in Google are eight times and two times higher, respectively, in the 28 countries with SARS cases in this two-week window than in other countries without SARS experience. People in countries without SARS experience pay attention to COVID-19 much later, when the disease begins to spread rapidly outside China in March. Furthermore, the number of Google searches for "SARS" is twelve times higher in the 10 countries with SARS deaths than in other countries without SARS deaths, suggesting that the imprint is stronger when the disease causes fatalities. In addition, earlier search attention to COVID-19 in countries with SARS imprints is prevalent within continent, even in non-Asian countries (e.g., countries in Europe and North America).

³⁴ See further details in the World Bank's report, [Global Economic Prospects](#).

Second, we examine how different search attention to COVID-19 affects stock market performance across countries during the first COVID-19 outbreak in Wuhan. In particular, we find that Google search attention for SARS is negatively associated with the cumulative abnormal returns (*CAR*) of stock market indexes from January 20 to January 31. Furthermore, a one standard deviation increase in Google search attention explained by the SARS imprint leads to a 61.35 basis point drop in *CAR*. This finding further supports the imprint channel we propose, since the investors imprinted by SARS react more strongly than other investors to the first COVID-19 outbreak. These findings complement those of Da et al. (2011, 2015), which show that investors' search attention plays a substantial role in the stock market.

Next, we examine the role of SARS imprints in government responses to domestic COVID-19 outbreaks. We perform a duration analysis of various government containment measures on the interactions between COVID-19 case numbers and the indicator denoting SARS experience. We find that contemporaneous COVID-19 case numbers are positively associated with the timeliness of containment measures (i.e., school closures, workplace closures, cancellation of public events, restrictions on public gatherings, restrictions on domestic movements, and international travel controls), and this effect is significantly more pronounced for countries with SARS imprints. For example, a 100% increase in COVID-19 cases is associated with 69.7% and 22.6% increases in the rates of school closures in countries with and without SARS infections, respectively. Furthermore, following government social distancing rules, people in countries with SARS deaths are also more constrained in their daily movements, which is associated with lower COVID-19 infection rates in such countries.

Our findings contribute to the literature examining the impact of prior experience on subsequent economic and social activities. Ever since the seminal work by Stinchcombe (1965), the social and economic impacts of imprints have been studied widely (e.g., Marquis and Tilcsik, 2013). A number of studies have shown that early life experiences can leave imprints that influence individuals' careers (e.g., Elder, 1986, 1998; Gibbons and Waldman, 2006; Oyer, 2006, 2008; Law and Zuo, 2020), risk attitude (e.g., Chiang et al., 2011; Malmendier and Nagel, 2011, 2016; Guiso et al., 2015; Bernile et al., 2017), investments (e.g., Kaustia and Knüpfer, 2008; Knüpfer et al., 2017; Huang, 2019; Malmendier et al., 2020),

and corporate management (e.g., Bayus and Agarwal, 2007; Billett and Qian, 2008; Malmendier et al., 2011; Kaplan et al., 2012; Benmelech and Frydman, 2015; Schoar and Zuo, 2017; He et al., 2018). In particular, inexperienced investors tend to neglect risk until they experience severe and adverse investment outcomes (e.g., Gennaioli et al., 2012; Chernenko et al., 2016). For the first time, this paper documents a crucial fundamental mechanism underlying the different responses to COVID-19 across the globe: the experience of similar viruses.³⁵ This has important policy implications for economic aid programs and containment measures worldwide, as early responses to COVID-19 can mean the difference between life and death.

2. Background

2.1 COVID-19 (caused by SARS-CoV-2)

According to the World Health Organization (WHO), severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2) causes COVID-19. The first known outbreak of COVID-19 was in Wuhan, China. The earliest known COVID-19 case, detected by SARS tests, was recorded on December 15, 2019. The media and public in China started to pay attention to COVID-19 in late January. In particular, on January 20, Dr. Zhong Nanshan, a Chinese epidemiologist who had earned international fame for managing the 2003 SARS outbreak in China, addressed the nation on China Central Television (CCTV) and, for the first time, confirmed the human-to-human transmission of COVID-19. Since then, the Chinese government has initiated several strong containment measures. On January 23, the Chinese government started a complete lockdown of Wuhan after 444 cases had been confirmed. This lockdown, which affected approximately 57 million people, was the first major move to contain the outbreak. On January 25, the first day of the Chinese Lunar New Year, the Standing Committee of the Politburo, the highest authority of the Chinese Communist Party, held an emergency meeting regarding COVID-19. It was unprecedented for the Standing Committee of the Politburo to convene on the Chinese New Year, so this meeting sent a strong signal of the severity of the COVID-19 situation to the rest of the world. On January 30, WHO declared COVID-19 a global

³⁵ Several contemporaneous studies examine the economic consequences of COVID-19 outbreaks in China and beyond, as well as policy responses to the virus. For example, see Atkeson (2020); Baker et al. (2020); Barrot et al. (2020); Chen et al. (2020); Ding et al. (2020); Duan et al. (2020); Eichenbaum et al. (2020); Fahlenbrach et al. (2020); Feng et al. (2020); Gormsen and Koijen (2020); Hassan et al. (2020); Ramelli and Wagner (2020); Stock (2020).

public health emergency. To examine people’s attention to the first known COVID-19 outbreak, we thus use the two-week window from January 20 to 31, which covers the first widespread media reports and government actions.³⁶

2.2 SARS (caused by SARS-CoV-1)

There are many similarities between viruses SARS-CoV-1 and SARS-CoV-2, which cause SARS disease and COVID-19 disease, respectively. Specifically, SARS-CoV-2 is most closely related to SARS-CoV-1, according to an article by the National Institutes of Health titled “SARS-CoV-2 stability similar to original SARS virus.” SARS-CoV-2 is recognized as a SARS family virus with similar symptoms and forms of transmission. Moreover, both first known outbreaks of SARS and COVID-19 were in mainland China. In summary, COVID-19 is similar to SARS in many respects but quite different from other known pathogens, such as MERS and the influenza virus.³⁷ Hence, in this paper, we use SARS infections to measure past adverse experience.

3. Data and Summary Statistics

We collect data on the 2003 SARS epidemic from the WHO website. The first SARS patient was identified in Guangdong province of China in November 2002, after which the disease spread to the other 28 countries. Ten out of these 28 countries across four continents reported SARS fatalities (see Table A1 in the Online Appendix). As of December 31, 2003, WHO had reported 8,096 SARS cases worldwide, with a fatality rate of 9.6%. We obtain data for the COVID-19 pandemic from WHO and Johns Hopkins University, whose databases cover daily COVID-19 confirmed cases and deaths for 165 countries/territories.

To measure Google search attention, we use two keywords: “SARS” and “coronavirus.” Google Trends provides search index measures ranging from zero (i.e., no search on the keyword) to 100 (i.e., peak popularity for the keyword), which represents search interests relative to the highest point for a given region and time. We obtain cross-sectional data for 165 countries from the “Interest by region” section to compare

³⁶ See Figure A1 in the Online Appendix for the important events and dates of COVID-19 developments worldwide, from the first known case in December 2019 to March 2020, when it became a global pandemic.

³⁷ See, for example, [“‘Sars-family’ virus claims the second victim in China”](#), *BBC*, January 16, 2020. Section 1.1 in the Online Appendix shows more detailed discussions for the similarities between SARS and COVID-19.

relative search intensities among regions. For countries with low search volumes, the search indexes are missing, and we replace them with zero. Panel A of Table 1 shows the summary statistics. From January 20 to 31, the average Google search index values for the keywords “SARS” and “coronavirus” are 2.848 and 9.930, respectively. Among $SARSCase=0$ countries, the average *GoogleSARS* and *GoogleCoronavirus* are 0.661 and 6.288, respectively. These numbers are much higher for $SARSCase=1$ countries (i.e., 13.554 and 27.750), especially for $SARSDeath=1$ countries (i.e., 25.6 and 33.2). This unconditional pattern suggests that people in countries with SARS imprints pay more attention to COVID-19-related information, especially people in countries with SARS deaths.

Table 1: Summary Statistics

<i>Panel A: Attention and Stock Returns</i>						
Variables	Sub-Sample				Full Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
	$SARSCase=0$	$SARSCase=1$	$SARSDeath=0$	$SARSDeath=1$	Mean	S.D.
<i>GoogleSARS</i>	0.661 (137)	13.554 (28)	1.381 (155)	25.600 (10)	2.848 (165)	9.439
<i>GoogleCoronavirus</i>	6.288 (137)	27.750 (28)	8.429 (155)	33.200 (10)	9.930 (165)	16.857
<i>CAR</i>	-0.827 (39)	-1.828 (25)	-0.902 (54)	-2.922 (10)	-1.218 (64)	1.891
<i>CumRet</i>	-2.226 (39)	-4.041 (25)	-2.540 (54)	-5.068 (10)	-2.935 (64)	2.295
<i>Panel B: Social Distancing</i>						
Variables	N	Mean	S.D.	Median	P25	P75
<i>School</i>	32,860	0.462	0.499	0	0	1
<i>WorkPlace</i>	32,860	0.142	0.349	0	0	0
<i>PublicEvent</i>	32,860	0.543	0.498	1	0	1
<i>Gathering</i>	32,860	0.274	0.446	0	0	1
<i>InternalMovement</i>	32,860	0.360	0.480	0	0	1
<i>Travel</i>	32,860	0.380	0.485	0	0	1
<i>COV19Cases</i>	32,860	26,178.500	163,558.900	234.5	0	4,410.5

Notes. This table presents the summary statistics of sample data. Panel A is for Google search indexes across 165 countries and stock market returns across 64 countries. Mainland China is excluded. Columns (1) and (2) show $SARSCase=0$ and 1 countries, respectively. Columns (3) and (4) show $SARSDeath=0$ and 1 countries, respectively. Columns (5) and (6) show the full sample. The mean values of *GoogleSARS*, *GoogleCoronavirus*, *CAR*, and *CumRet* are reported with observation numbers in parentheses below. Panel B is for the country-date panel data on government social distancing policies and domestic COVID-19 development across 155 countries from January 1 to July 30, 2020. See the Appendix Table for detailed variable definitions.

Moreover, we obtain information for government social distancing policies from the Oxford COVID-19 Government Response Tracker (OxCGRT) for the following six categories: school closure, workplace closure, public events cancellation, gathering restrictions, restrictions on internal movement between cities/regions, and international travel controls (Hale et al., 2020). As shown in Panel B of Table 1, closing schools and canceling public events are the most frequently implemented government policies, while closing workplaces is the least frequently implemented policy. Of the 155 countries in our sample, 147 countries implemented school closures at all levels, and 99 countries implemented workplace closures for all non-essential industries.

4. Empirical Analyses and Results

4.1 The Imprint of the 2003 SARS Epidemic

We begin our analysis by estimating whether an imprint of the 2003 SARS experience exists during the initial COVID-19 outbreak in Wuhan. Specifically, we perform an OLS regression of Google search indexes for SARS during the two-week window from January 20 to January 31, 2020, on the country's SARS experience. This two-week window covers the entire first government response in China and broad media coverage of COVID-19, as described in Section 2.1.³⁸ Formally, the regression can be expressed as follows:

$$GoogleSARS_i = \alpha + \beta \times SARSCase_i + Controls_i + Fixed\ Effects + \varepsilon_i, \quad (1)$$

where $GoogleSARS_i$ indicates the Google search index for the keyword “SARS” in country i during the initial outbreak in Wuhan; $SARSCase_i$ is an indicator variable denoting that country i has recorded domestic SARS cases. Panel A of Table 2 shows the results. In column (1), the coefficient on $SARSCase$ is 5.282 at the 5% significance level. Given that the average $GoogleSARS$ among $SARSCase=0$ countries is 0.661, the search attention for SARS-related information is about eight times ($5.282/0.661=7.991$) higher in countries with SARS experience.

³⁸ Table A2 in the Online Appendix shows similar results for different time windows of the outbreak in Wuhan.

Furthermore, we use $SARSDeath_i$ as the main independent variable in the regression, an indicator variable denoting that country i has recorded domestic SARS deaths. In column (3), the coefficient on $SARSDeath$ is 16.477 at the 1% significance level, suggesting that people in countries with SARS fatalities search about twelve times ($16.477/1.381=11.931$) more intensively than people in countries without SARS fatalities. These results suggest that the 2003 SARS experience left an imprint in the memories of people who started to search for SARS information at the very beginning of the COVID-19 outbreak in China, and the experience of SARS deaths left an even stronger imprint. In addition, people in countries without SARS deaths started to pay attention to SARS and COVID-19-related information much later, at the beginning of March, when COVID-19 spread widely outside of China (see Figure A2 in the Online Appendix).

In Panel B, we use “coronavirus” as the Google search keyword to estimate the attention to COVID-19-related information in late January, since WHO did not officially name the pandemic “COVID-19” until March 11, 2020. In column (1), the coefficient on $SARSCase$ is 12.679 at the 1% significance level, suggesting that people in countries with SARS cases search for coronavirus-related information about two times ($12.679/6.288=2.016$) more intensively than people in countries without SARS experience.

Table 2: Imprints of SARS (Google Search Indexes)

	Full Sample				Excluding Asia	
<i>Panel A: SARS searches</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>
Variables	<i>SARS</i>	<i>SARS</i>	<i>SARS</i>	<i>SARS</i>	<i>SARS</i>	<i>SARS</i>
<i>SARSCase</i>	5.282** (2.33)	5.527** (2.29)			5.167* (1.75)	
<i>SARSDeath</i>			16.477*** (3.37)	16.595*** (3.34)		13.721* (1.86)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	NO	YES	NO	YES	YES	YES
Observations	165	165	165	165	124	124
Adjusted R ²	0.562	0.554	0.648	0.641	0.340	0.522

	Full Sample				Excluding Asia	
<i>Panel B: Coronavirus searches</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>	<i>Google</i>
Variables	<i>Coronavirus</i>	<i>Coronavirus</i>	<i>Coronavirus</i>	<i>Coronavirus</i>	<i>Coronavirus</i>	<i>Coronavirus</i>
<i>SARSCase</i>	12.679*** (2.72)	15.469*** (3.69)			18.516*** (4.29)	
<i>SARSDeath</i>			17.878** (2.17)	20.824*** (2.84)		17.071*** (3.06)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	NO	YES	NO	YES	YES	YES
Observations	165	165	165	165	124	124
Adjusted R ²	0.361	0.509	0.355	0.497	0.636	0.586

Notes. This table presents the results of cross-sectional OLS regressions of Google search indexes on SARS imprints during the initial COVID-19 outbreak in Wuhan. In Panel A (B), the dependent variable, *GoogleSARS* (*GoogleCoronavirus*), is the Google search index for keyword “SARS” (“coronavirus”) from January 20 to 31, 2020. The main independent variable, *SARSCase* (*SARSDeath*), is an indicator denoting the country that had SARS cases (deaths). *Log(GDP)*, *Log(Popu)*, *Log(AvgCOV19)*, *TradeIntensity*, *LifeExpectancy*, and *Log(GovDebt)* are controlled in all columns. Mainland China is excluded. See the Appendix Table for detailed variable definitions. Robust standard errors are used, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

In addition to the imprint hypothesis, several alternative mechanisms may explain the higher search attention for SARS. First, the countries that recorded SARS infections might be more vulnerable to COVID-19 as well, given the similarity between the two viruses, which were also both discovered in mainland China. To mitigate this concern, we exclude mainland China from our analyses to estimate other countries’ responses before COVID-19 spread globally. Google is also restricted in mainland China.

Second, 13 of the 28 countries that suffered from SARS in 2003 are in Asia and are geographically close to mainland China. Those countries could be more seriously affected by COVID-19. We control for the average number of daily new COVID-19 cases in this window, which measures the severity of the domestic COVID-19 situation. Only 4 out of the 165 countries had more than 10 confirmed COVID-19 cases as of January 31, 2020. Furthermore, we control for life expectancy to proxy for the robustness of a country’s healthcare systems, the size of the government debt to proxy for the government’s fiscal capacity to combat COVID-19, GDP, and population.³⁹ We also control for countries’ trade intensity with China to mitigate the concern that economic proximity to mainland China could explain the higher attention to

³⁹ We thank the editorial team for these useful suggestions.

COVID-19.

Third, in columns (2) and (4), to further mitigate the concern of geographic proximity to mainland China, we include continent fixed effects to use the variation within continent and find consistent results. Moreover, in columns (5) and (6), we exclude Asian countries and restrict our sample to the other continents, mainly countries in Europe, North America, and Africa. The coefficients on *SARSCase* and *SARSDeath* are both significantly positive. These findings serve as strong evidence that even in non-Asian countries such as Europe, people in countries affected by SARS in 2003 pay significantly greater attention to the COVID-19 outbreak in Wuhan even when there were zero (or close to zero) domestic cases.⁴⁰

4.2 Attention, Stock Market Reaction, and SARS Imprints

This section further examines the relation between the increased search attention in Google and the stock market performance at the beginning of the pandemic. Specifically, we obtain stock market index data from Thomson Reuters Eikon for the biggest 65 markets, which comprise more than 99% of the total global stock market capitalization in 2018. We choose the MSCI World Price Index as the global market index.

Following the seminal work by Da et al. (2011, 2015), we regress *CAR* from January 20 to January 31, 2020 on Google search indexes for SARS. Table 3 reports the regression results. In column (1), the coefficient on *GoogleSARS* is -0.044 at the 5% significance level, suggesting that increased search attention is associated with decreases in the stock market. In column (2), we calculate the cumulative return and obtain similar results.

Next, we explore whether the SARS imprint explains the association between Google search attention and stock market returns. For these 64 countries, we repeat the regression in column (2) of Table 2 Panel A and calculate the predicted *GoogleSARS*. Then, we perform the regression of *CAR* on predicted *GoogleSARS*. In column (3) of Table 3, the coefficient on $\widehat{GoogleSARS}_{Case}$ is -0.065, suggesting that a

⁴⁰ Following Ding et al. (2020a), we compare Google search attention before and after the date at which each country surpassed 100 COVID-19 cases. Table A3 in the Online Appendix shows that people in countries with SARS cases pay significantly more attention to COVID-19 following domestic outbreaks.

one standard deviation increase in Google search attention explained by the SARS imprint leads to a 61.35 (9.439×0.065) basis point drop in CAR. In column (4), we use *SARSDeath* to predict *GoogleSARS* and find that the coefficient on $\widehat{GoogleSARS}_{Death}$ is -0.074, which is larger than that in column (3), suggesting a deeper imprint resulting from SARS deaths and a stronger stock market reaction. In columns (5) and (6), we again use cumulative return as dependent variable and obtain similar results.

Table 3: SARS Experience, Attention, and Return

Variables	(1) CAR	(2) CumRet	(3) CAR	(4) CAR	(5) CumRet	(6) CumRet
<i>GoogleSARS</i>	-0.044** (-2.63)	-0.049* (-1.80)				
$\widehat{GoogleSARS}_{Case}$			-0.065* (-1.75)		-0.123** (-2.20)	
$\widehat{GoogleSARS}_{Death}$				-0.074*** (-3.07)		-0.142*** (-3.60)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	YES	YES	YES	YES	YES	YES
Observations	64	64	64	64	64	64
Adjusted R ²	0.469	0.362	0.433	0.453	0.364	0.416

Notes. This table presents the results of cross-sectional OLS regressions of stock market returns during the initial COVID-19 outbreak in Wuhan on SARS imprints for 64 markets (mainland China is excluded). The dependent variable, *CAR* (*CumRet*), is the cumulative abnormal return (cumulative return) from January 20 to 31, 2020. $\widehat{GoogleSARS}_{Case}$ ($\widehat{GoogleSARS}_{Death}$) is the predicted Google search index using *SARSCase* (*SARSDeath*). *Log(GDP)*, *Log(Popu)*, *Log(AvgCOV19)*, *TradeIntensity*, *LifeExpectancy*, and *Log(GovDebt)* are controlled in all columns. See the Appendix Table for detailed variable definitions. Robust standard errors are used, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

4.3 Government Actions to Combat COVID-19 Outbreaks

In this section, we aim to understand the impact of SARS imprints on government actions to combat domestic COVID-19 outbreaks. Figure 1 shows the scatter plot of Google search indexes for “SARS” from January 20 to January 31, 2020, versus the cumulative COVID-19 cases per million population on the 90th day since the first confirmed domestic case. The countries with SARS deaths are clustered in the bottom-right corner, suggesting that the SARS imprint is associated with higher search attention and lower COVID-19 infections.

Figure 1: COVID-19 Development, Google Search, and SARS Imprints

Notes. This figure shows the scatter plot of COVID-19 development and Google search attention across 64 countries/territories (mainland China is excluded). The vertical axis displays the cumulative number of COVID-19 cases per million population on the 90th day since the first confirmed domestic case. The horizontal axis displays the natural logarithm of one plus the Google search index for the keyword “SARS” from January 20 to 31, 2020. Diamonds (circles) denote countries/territories with (without) SARS deaths.

One of the major criticisms of government containment measures is the slow response to COVID-19. We employ the Cox proportional hazard model to study the association between the likelihood of implementing various containment measures (e.g., lockdowns) and the SARS imprint. Formally, the regression can be expressed as follows:

$$h_i(t) = h_0(t) \exp(\beta_1 \times \text{Log}(\text{COV19Cases})_{i,t} + \beta_2 \times \text{Log}(\text{COV19Cases})_{i,t} \times \text{SARSCase}_i + \beta_3 \times \text{SARSCase}_i + \text{Controls}_i + \text{Continent FE}), \quad (2)$$

where $h_i(t)$ is the expected hazard at date t for country i ; $h_0(t)$ is the baseline hazard and represents the hazard when all of the predictors are equal to zero; $\text{Log}(\text{COV19Cases})_{i,t}$ is the natural logarithm of one plus the cumulative number of confirmed COVID-19 cases in country i on date t ; Controls_i includes life expectancy, government debt, GDP, population, and trade intensity with China. We also control for continent fixed effects. Each country enters the hazard regression on the date of its first confirmed domestic COVID-19 case and exits after the respective policy takes effect. We cluster the standard errors by date to allow the correlation of errors across countries.

The sample contains 155 countries worldwide for which we have data on government actions. We estimate the hazard probabilities for six different containment measures in Table 4. Panel A uses *SARSCase* to proxy for the SARS imprint, and Panel B uses *SARSDeath*. In columns (1) to (6) of Panel A, the coefficients on $\text{Log}(\text{COV19Cases})$ are all positive, suggesting that the likelihood of enforcing these containment measures is higher when there are more contemporaneous COVID-19 cases. Moreover, in columns (1) to (6), the coefficients on $\text{Log}(\text{COV19Cases}) \times \text{SARSCase}$ are all significantly positive, suggesting that the positive associations between COVID-19 case number and the likelihood of containment measures are higher in countries with SARS imprints: governments with SARS experience respond more quickly to domestic outbreaks than governments without SARS experience.

For example, for school closure in column (1), a 100% increase in the COVID-19 case number yields a hazard ratio equal to 1.226 ($\text{Exp}(0.294 \times \text{Ln}(2)) = 1.226$) in countries without SARS experience, indicating that the rate of school closure increases by 22.6%. For countries with SARS cases, a 100% increase in the

COVID-19 case number leads to a 69.7% increase in the rate of school closure ($\text{Exp}((0.294+0.469) \times \ln(2)) - 1$).

In Panel B, we use *SARSDeath* to proxy for the SARS imprint and find similar but stronger results. These findings are consistent with Table 2, which shows that the imprint of SARS experience, especially SARS deaths, plays an important role in how a country later copes with similar crises. Again, we restrict the sample to non-Asian countries and find consistent results for all six containment measures (see Table A4 in the Online Appendix).

Table 4: Policy Responses to Combat COVID-19

Variables	(1) <i>School</i>	(2) <i>WorkPlace</i>	(3) <i>Public Event</i>	(4) <i>Gathering</i>	(5) <i>Internal Movement</i>	(6) <i>Travel</i>
<i>Panel A: SARS Case</i>						
<i>Log(COVI9Cases)</i>	0.294*** (4.15)	0.202*** (3.02)	0.393*** (3.42)	0.040 (0.70)	0.225*** (3.61)	0.122 (1.49)
<i>Log(COVI9Cases)×SARSCase</i>	0.469*** (5.60)	0.405*** (5.89)	0.341*** (3.45)	0.437*** (5.48)	0.261*** (3.15)	0.197*** (3.25)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	YES	YES	YES	YES	YES	YES
Observations	3,060	11,036	3,033	10,121	6,420	7,551
Chi-Squared	127.9	147.2	119.5	118.6	63.62	66.96
<i>Panel B: SARS Death</i>						
<i>Log(COVI9Cases)</i>	0.373*** (5.00)	0.273*** (4.40)	0.459*** (4.23)	0.101** (2.12)	0.283*** (4.93)	0.167** (2.22)
<i>Log(COVI9Cases)×SARSDeath</i>	0.608** (2.38)	1.138*** (5.25)	0.773*** (3.36)	0.485*** (3.24)	0.748*** (3.58)	0.637*** (4.89)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	YES	YES	YES	YES	YES	YES
Observations	3,060	11,036	3,033	10,121	6,420	7,551
Chi-Squared	122.8	173.8	96.41	70.57	87.80	64.92

Notes. This table presents the results of Cox proportional hazard regressions for policy responses to COVID-19 at the country-date level. Each country enters the hazard regression (origin date) when the country reports its first COVID-19 case. The failure date is the date when the respective policy takes effect. *Log(COVI9Cases)* is the natural logarithm of one plus the number of concurrent cumulative confirmed cases. *SARSCase* (*SARSDeath*) is an indicator denoting the country had SARS cases (deaths). *SARSCase* (*SARSDeath*), *Log(GDP)*, *Log(Popu)*, *TradeIntensity*, *LifeExpectancy*, and *Log(GovDebt)* are controlled in all columns. Mainland China is excluded. See the Appendix Table for detailed variable definitions. Standard errors are clustered at the calendar date level, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 2 shows that people's daily transit movements are substantially lower in countries with SARS deaths (i.e., the dashed line) than in other countries without SARS deaths (i.e., the solid line) during both

the beginning of the pandemic (January to early March) and the reopening stages (May to July). Many countries started to implement social distancing rules in March and relaxed those rules gradually beginning in May. In summary, both governments and individuals with SARS imprints start social distancing earlier and are more cautious in the reopening stage.⁴¹ This could explain the lower infection rates in such countries, as suggested in Figure 1.

Figure 2: SARS Experience and Residential Mobility

Notes. This figure shows the average transit movement trend for two groups of countries from January 13 to July 30, 2020, based on Apple mobility data. The vertical axis represents the daily number of requests made to Apple Maps for directions by transportation type “Transit,” which is normalized to 100 on January 13, 2020 for each country. The dashed (solid) line represents the countries with (without) SARS deaths. Figure A3 in the Online Appendix shows the pattern for transportation types “Walking” and “Driving.”

5. Conclusion

The current COVID-19 pandemic is a once-in-a-century global crisis. A universally accepted tenet of public health is that healthcare systems should respond to pandemics as early and as intensively as possible. Although many countries have been working hard to combat this disease, COVID-19 has nevertheless spread dramatically in many parts of the world, and its impacts are detrimental to human life and well-

⁴¹ Table A5 in the Online Appendix shows similar results obtained via multivariate analysis.

being. While we document the impact of SARS experience on individual and governmental responses to COVID-19, understanding the long-term consequences of this pandemic experience is left for future research.

Appendix Table: Variable Definitions

Variable Names	Variable Definitions
<i>SARSCase</i>	An indicator variable that equals one if the country had SARS cases, and zero otherwise.
<i>SARSDeath</i>	An indicator variable that equals one if the country had SARS deaths, and zero otherwise.
<i>GoogleSARS</i>	Google search index for keyword “SARS” from January 20 to January 31, 2020. For countries with low search volumes, the search indexes are missing, and we replace them with zero.
<i>GoogleCoronavirus</i>	Google search index for keyword “coronavirus” from January 20 to January 31, 2020. For countries with low search volumes, the search indexes are missing, and we replace them with zero.
<i>Log(GDP)</i>	Natural logarithm of the country’s GDP (in USD). (World Bank Database.)
<i>Log(Popu)</i>	Natural logarithm of the country’s total population. (World Bank Database.)
<i>Log(AvgCOV19)</i>	Natural logarithm of one plus the average daily new COVID-19 cases from January 20 to January 31, 2020.
<i>CAR</i>	Cumulative abnormal return, computed as the sum of abnormal daily returns (in percentage points) from January 20 to January 31, 2020, where the abnormal return is calculated based on the market model: $Abnormal\ Return_{i,t} = Ret_{i,t} - \hat{\alpha}_i - \hat{\beta}_{1i,t-1}Ret_{M,t-1} - \hat{\beta}_{2i}Ret_{M,t} - \hat{\beta}_{3i}Ret_{M,t+1}$, where $\hat{\alpha}_i$, $\hat{\beta}_{1i}$, $\hat{\beta}_{2i}$, and $\hat{\beta}_{3i}$ are estimated in the pre-event window period from July 1 to December 31, 2019, $Ret_{i,t}$ is the index return in country/territory i on date t , and $Ret_{M,t}$ is the return of the global market index (i.e., the MSCI World Price Index).
<i>CumRet</i>	Cumulative return computed as the sum of daily returns (in percentage points) of the stock index from January 20 to January 31, 2020. (Thomson Reuters Eikon Database.)
<i>TradeIntensity</i>	The ratio between the country’s trade amount with China and its total trade amount calculated using 2018 data, where trade amount equals the sum of imports and exports. (United Nations ComTrade Database.)
<i>LifeExpectancy</i>	The average number of years an individual in the country is expected to live. (United Nations Population Division Database.)
<i>Log(GovDebt)</i>	Natural logarithm of one plus the country’s government debt amount (in USD). (IMF Database.)
<i>School</i>	An indicator variable that equals one when the school closure policy is enforced at all levels in the country, and zero before the policy takes effect.
<i>Workplace</i>	An indicator variable that equals one when the work-from-home policy is enforced for all but essential workplaces in the country, and zero before the policy takes effect.
<i>PublicEvent</i>	An indicator variable that equals one when the country requires public events to be canceled, and zero before the policy takes effect.
<i>Gathering</i>	An indicator variable that equals one when the country bans gatherings of ten people or more, and zero before the policy takes effect.
<i>InternalMovement</i>	An indicator variable that equals one when the country restricts domestic transit between cities/regions, and zero before the policy takes effect.
<i>Travel</i>	An indicator variable that equals one when the country closes its border (i.e., an international travel ban), and zero before the policy takes effect.
<i>Log(COV19Cases)</i>	Natural logarithm of one plus the country’s cumulative number of COVID-19 cases at date t .

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Online Appendix for

Combating the COVID-19 Pandemic: The Role of the SARS Imprint

Additional Background of COVID-19

1.1 Government Responses to COVID-19

The coronavirus disease 2019 (COVID-19) is the sixth and the most recent global deadly pandemic.⁴² By December 31, 2020, more than 83 million confirmed COVID-19 cases have been reported in over 200 countries, areas, or territories, with more than 1.8 million recorded deaths related to COVID-19.⁴³ Bill Gates, the co-chairman and co-founder of the Bill & Melinda Gates Foundation that heavily invested in global healthcare systems, said that the COVID-19 is starting to behave like the “once-in-a-century pathogen we’ve been worried about.”

The United States, the hot zone of COVID-19, has been criticized heavily for its slow responses to contain the virus. For example, the Center for Disease Control (CDC) test kits were found to have manufacturing defects, and the Food and Drug Administration (FDA) did not allow the use of any test kits from institutions other than CDC until February 29.⁴⁴ As of December 2020, nearly 20 million COVID-19 cases with more than 345 thousand deaths have been reported in the U.S., according to Coronavirus Resource Center at Johns Hopkins University. In an article from *Science*, IHME COVID-19 Forecasting Team predicted that around 511,000 lives could be lost to COVID-19 in the U.S. alone by the end of February in 2021. See, “Modeling COVID-19 scenarios for the United States”, *Science*, October 23, 2020.

As a part of the urgent plans to respond to COVID-19, leaders of G20 major economies held a virtual talk on March 31, 2020, claiming to inject US\$ 5 trillion fiscal stimulus. Kristalina Georgieva, Managing Director of IMF, stated that nearly 80 countries are requesting IMF to help battle coronavirus, and IMF is ready to deploy US\$1 trillion lending capacity. See, “Understanding the economic shock of coronavirus”,

⁴² The past five notable pandemics in modern human history are the Plague of Justinian in the 6th century, Black Death in the 14th century, Smallpox from the 15th to the 17th century, Cholera in the early 19th century, and the 1918 influenza pandemic.

⁴³ Our COVID-19 data in the analyses are obtained from WHO and Johns Hopkins University (JHU).

⁴⁴ Kelly Wroblewski, director of infectious disease at the Association of Public Health Laboratories, stated that the critical problem with CDC’s test kits is the negative control. The declaration of public health emergencies, unintentionally, limited the diagnostic capacity. See detailed description in a news article from *Science*: <https://www.sciencemag.org/news/2020/02/united-states-badly-bungled-coronavirus-testing-things-may-soon-improve#>.

Harvard Business Review, March 27, 2020; “Coronavirus: A visual guide to the economic impact”, *BBC*, June 30, 2020.

1.2 COVID-19 and Other Epidemics

The novel coronavirus is recognized as the SARS family virus. Chan et al. (2020), published online on January 24, 2020, studied the familial cluster of pneumonia associated with the novel coronavirus and conducted a phylogenetic analysis showing it is closest to the SARS-related coronavirus found in Chinese horseshoe bats. Moreover, Van Doremalen et al. (2020) found that the stability of SARS-CoV-2 in aerosols and on various surfaces is similar to that of SARS-CoV-1.

Positive coronavirus was first detected using the SARS tests in the Central Hospital of Wuhan. One doctor, Li Wenliang, shared a message to fellow doctors on December 30, 2019, warning about a possible outbreak of an illness which resembled severe acute respiratory syndrome in Wuhan (“Li Wenliang”, *The Lancet*, February 18, 2020). Subsequently, there were 27 COVID-19 cases recorded as of December 31, 2019. On January 9, 2020, a 61-year-old man, who had an underlying heart condition, became the first known person to die of COVID-19. The virus spread rapidly across cities in China, and by March 15, 2020, there were 80,860 officially confirmed cases, as shown in Figure A4.⁴⁵ The first confirmed case outside of China, according to the WHO, was reported in Thailand on January 13, followed by a few confirmed cases in other countries. Figure A4 shows that the total number of confirmed cases outside mainland China was under 150 until January 31.

In addition to SARS, several other epidemics happened in the past 20 years, such as the Middle East respiratory syndrome (MERS) caused by MERS-CoV and the swine influenza caused by H1N1. However, they are quite different from COVID-19. In particular, the MERS outbreak started in Saudi Arabia in 2012 and resulted in 2,494 laboratory-confirmed cases with a total of 858 deaths (34% fatality rate) at the end of November 2019.⁴⁶ The fatality rates and the speeds of transmission are very different between COVID-19 and MERS. Moreover, WHO particularly pointed out the key differences between influenza and COVID-

⁴⁵ See Huang et al. (2020) on the development of coronavirus in Wuhan, China.

⁴⁶ <https://www.who.int/emergencies/mers-cov/en/>.

19. For example, influenza has a much shorter incubation period and spreads faster than COVID-19. More importantly, the fatality of influenza is below 0.1% (e.g., 0.02% for H1N1, according to the CDC), while the crude fatality rate of COVID-19 is between 3% to 4%, according to WHO.⁴⁷

The relationship between SARS and COVID-19 is not just about virus similarity. The experience gained from SARS also helps doctors to combat the COVID-19. David Naylor, a leading Canadian expert on pandemic control, said that Canada's response to COVID-19 benefited vastly from the country's experience with the 2003 SARS epidemic that killed 44 Canadians (Webster, 2020). In contrast, the U.S. has recorded SARS cases but no deaths and has approximately four times as many COVID-19 cases per million population and more than twice as many COVID-19 deaths per million population as Canada by December 2020.

⁴⁷ <https://www.who.int/news-room/q-a-detail/q-a-similarities-and-differences-covid-19-and-influenza>.

Figure A1: Timeline of COVID-19 Outbreak

Notes. This figure shows the milestones for COVID-19 development worldwide from the first known case in December 2019 to March 2020, when it became the global pandemic. We summarize these events with the dates accordingly along the timeline.

Figure A2: Trend of Google Search Index

Panel A: SARS Searches

Panel B: Coronavirus Searches

Notes. This figure shows the average Google search index for two groups of countries from January 1 to May 1, 2020. In Panel A, the vertical axis represents the Google search index for the keyword “SARS.” In Panel B, the vertical axis represents the Google search index for the keyword “coronavirus.” Google search index for each country and each keyword is downloaded separately from <https://trends.google.com>. Google normalized the search results to make it comparable across countries. The daily Google search index for each country during a certain period equals the total number of searches in this day divided by the peak number of searches of this country over this period. The Google search index ranges from zero (i.e., no search on the keyword) to 100 (i.e., the peak number of searches for the keyword), representing relative search interests compared to the highest point in the period. More details on the data can be found at https://support.google.com/trends/answer/4365533?hl=en&ref_topic=6248052. In both panels, the dashed line represents the average Google search index in the 10 countries with SARS deaths. The solid line represents the average Google search index in the 155 countries without SARS deaths. Mainland China is excluded.

Figure A3: SARS Experience and Residential Mobility (Robustness)

Panel A: Mobility trend for Walking

Panel B: Mobility trend for Driving

Notes. This figure shows the average movement trend for two groups of countries from January 13 to July 30, 2020, based on [Apple mobility data](https://www.apple.com/covid19/mobility). In Panel A, the vertical axis represents the daily number of requests made to Apple Maps for directions by transportation type “Walking,” which is normalized to 100 on January 13, 2020, for each country. In Panel B, the vertical axis represents the daily number of requests made to Apple Maps for directions by transportation type “Driving,” which is normalized to 100 on January 13, 2020, for each country. Data for May 11 and May 12 are not available from Apple, and hence we use linear interpolation to fill up the two-day values. Mainland China is not covered in the data. More details on the data can be found at <https://www.apple.com/covid19/mobility>. The dashed (solid) line represents the countries with (without) SARS deaths.

Figure A4: COVID-19 Case Numbers Over Time

Notes. This figure shows the cumulative number of COVID-19 cases across the globe from January 16 to March 15, 2020. The green solid line represents the cumulative number of COVID-19 cases in the Hubei province of China. The blue dashed line represents the cumulative number of COVID-19 cases in other parts of mainland China. The yellow dotted line represents the cumulative number of COVID-19 cases outside mainland China. Starting from February 12, 2020, Hubei province started to include clinically diagnosed cases into total confirmed cases, which caused a sharp increase in the cumulative number of COVID-19 cases. While in other places, only lab-confirmed cases are included in the WHO data.

Table A1: Reported SARS Cases Around the World

Country/Territory	Female	Male	Total	Number of deaths	Case fatality ratio (%)
Australia	4	2	6	0	0
Canada	151	100	251	43	17
France	1	6	7	1	14
Germany	4	5	9	0	0
Hong Kong SAR	977	778	1,755	299	17
India	0	3	3	0	0
Indonesia	0	2	2	0	0
Italy	1	3	4	0	0
Kuwait	1	0	1	0	0
Macao SAR	0	1	1	0	0
Mainland China	2,674	2,607	5,327	349	7
Malaysia	1	4	5	2	40
Mongolia	8	1	9	0	0
New Zealand	1	0	1	0	0
Philippines	8	6	14	2	14
Ireland	0	1	1	0	0
South Korea	0	3	3	0	0
Romania	0	1	1	0	0
Russia	0	1	1	0	0
Singapore	161	77	238	33	14
South Africa	0	1	1	1	100
Spain	0	1	1	0	0
Sweden	3	2	5	0	0
Switzerland	0	1	1	0	0
Taiwan	218	128	346	37	11
Thailand	5	4	9	2	22
United Kingdom	2	2	4	0	0
United States	13	14	27	0	0
Vietnam	39	24	63	5	8
Total			8,096	774	9.6

Notes. This table presents the information about SARS spread. The list contains all countries/territories with SARS cases. We report the total number of SARS cases (female vs. male), the number of deaths, and the fatality ratio (in percentage point). Data are obtained from the last update of the WHO summary of the cumulative SARS report, which is available at https://www.who.int/csr/sars/country/table2004_04_21/en/.

Table A2: Imprints of SARS (Longer Windows)

	(1)	(2)	(3)	(4)	(5)	(6)
Time Window	[Jan 07, Jan 31]	[Jan 07, Jan 31]	[Jan 20, Feb 20]	[Jan 20, Feb 20]	[Jan 07, Feb 20]	[Jan 07, Feb 20]
Variables	<i>GoogleSARS</i>	<i>GoogleSARS</i>	<i>GoogleSARS</i>	<i>GoogleSARS</i>	<i>GoogleSARS</i>	<i>GoogleSARS</i>
<i>Panel A: SARS Case</i>						
<i>SARSCase</i>	9.187**	9.605**	9.134**	9.504**	10.376**	10.800**
	(2.15)	(2.12)	(2.36)	(2.32)	(2.31)	(2.28)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	NO	YES	NO	YES	NO	YES
Observations	165	165	165	165	165	165
Adjusted R ²	0.499	0.490	0.412	0.401	0.393	0.382
<i>Panel B: SARS Death</i>						
<i>SARSDeath</i>	27.963***	28.276***	26.003***	26.282***	29.069***	29.396***
	(2.66)	(2.65)	(2.87)	(2.86)	(2.80)	(2.80)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	NO	YES	NO	YES	NO	YES
Observations	165	165	165	165	165	165
Adjusted R ²	0.616	0.610	0.573	0.565	0.568	0.560

Notes. This table presents the results of the cross-sectional OLS regressions of Google search indexes on SARS experience in three different time windows, covering the initial COVID-19 outbreak in Wuhan. The time window in columns (1) and (2) is from January 7 to January 31, 2020. The time window in columns (3) and (4) is from January 20 to February 20, 2020. The time window in columns (5) and (6) is from January 7 to February 20, 2020. China announced that a new type of coronavirus (i.e., novel coronavirus) was identified on January 7. The human-to-human transmission of COVID-19 was confirmed on January 20. February 20 is right before the COVID-19 outbreak in South Korea, Iran, and Europe (i.e., non-SARS countries). The dependent variable, *GoogleSARS*, is the Google search index for keyword “SARS” during the respective time windows. In Panel A (B), the independent variable, *SARSCase* (*SARSDeath*), is an indicator denoting that the country had SARS cases (deaths). *Log(GDP)*, *Log(Popu)*, *Log(AvgCOV19)*, *TradeIntensity*, *LifeExpectancy*, and *Log(GovDebt)* are controlled in all columns. *Log(AvgCOV19)* is the natural logarithm of one plus the average daily new COV-ID 19 cases during the respective time windows. Mainland China is excluded. See the Appendix Table for detailed variable definitions. Robust standard errors are used, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3: Google Search, COVID-19 Development, and SARS Imprints

Variables	(1) [-5, 5] <i>Google Coronavirus</i>	(2) [-10, 10] <i>Google Coronavirus</i>	(3) [-15, 15] <i>Google Coronavirus</i>
<i>Post</i>	2.686** (1.99)	4.351*** (2.91)	5.799*** (3.82)
<i>Post</i> × <i>SARSCase</i>	9.810*** (2.90)	17.118*** (5.32)	23.451*** (6.82)
Country FE	YES	YES	YES
Observations	1,757	3,352	4,944
Adjusted R ²	0.702	0.584	0.485

Notes. This table presents the results of the panel OLS regressions of Google search indexes on COVID-19 development and SARS experience. We construct a balanced panel of 165 countries from January 1 to July 31, 2020. Following Ding et al. (2020), in columns (1), (2), and (3), we restrict the regression sample to observations during 5, 10, and 15 days before and after the discovery of 100 confirmed COVID-19 cases in each country. The dependent variable, *GoogleCoronavirus*, is the daily Google search index for the keyword “coronavirus” for each country. *Post* is an indicator denoting the dates after the discovery of 100 COVID-19 cases. *SARSCase* is an indicator denoting that the country had SARS cases. Mainland China is excluded. Standard errors are clustered at the country level, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Policy Responses to Combat COVID-19 (Exclude Asia)

Variables	(1) <i>School</i>	(2) <i>WorkPlace</i>	(3) <i>Public Event</i>	(4) <i>Gathering</i>	(5) <i>Internal Movement</i>	(6) <i>Travel</i>
<i>Panel A: SARS Case</i>						
<i>Log(COV19Cases)</i>	0.728*** (6.80)	0.251*** (2.64)	0.428*** (2.93)	0.086 (1.26)	0.332*** (3.45)	0.220** (2.04)
<i>Log(COV19Cases)×SARSCase</i>	0.457*** (2.98)	0.488*** (3.93)	0.451*** (3.29)	0.436*** (4.50)	0.244** (2.03)	0.182** (2.23)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	YES	YES	YES	YES	YES	YES
Observations	2,304	7,977	1,951	7,627	4,157	5,047
Chi-Squared	170	135.5	77.23	103.4	50.96	107.2
<i>Panel B: SARS Death</i>						
<i>Log(COV19Cases)</i>	0.882*** (7.45)	0.347*** (4.14)	0.534*** (4.20)	0.187*** (3.49)	0.402*** (4.41)	0.279*** (2.94)
<i>Log(COV19Cases)×SARSDeath</i>	2.553*** (4.09)	1.298*** (3.00)	8.156*** (3.95)	0.339** (2.00)	1.053*** (3.21)	1.091** (2.48)
Controls	YES	YES	YES	YES	YES	YES
Continent FE	YES	YES	YES	YES	YES	YES
Observations	2,304	7,977	1,951	7,627	4,157	5,047
Chi-Squared	127.6	181.9	116.7	49.39	73.24	44.71

Notes. This table presents the results of Cox proportional hazard regressions for policy responses to COVID-19 at the country-date level. Asian countries are excluded in all columns. Each country enters the hazard regression (origin date) when the country reports its first COVID-19 case. The failure date is the date when the respective policy takes effect. *Log(COV19Cases)* is the natural logarithm of one plus the number of concurrent cumulative confirmed cases. *SARSCase* (*SARSDeath*) is an indicator denoting that the country had SARS cases (deaths). *SARSCase* (*SARSDeath*), *Log(GDP)*, *Log(Popu)*, *TradeIntensity*, *LifeExpectancy*, and *Log(GovDebt)* are controlled in all columns. See the Appendix Table for detailed variable definitions. Standard errors are clustered at the calendar date level, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A5: SARS Experience and Mobility

Variables	(1) <i>Stay-at-Home</i>	(2) <i>Stay-at-Home</i>	(3) <i>Transit</i>	(4) <i>Transit</i>	(5) <i>Go to Workplace</i>	(6) <i>Go to Workplace</i>
<i>PolicyStringency</i>	1.025*** (29.69)	1.031*** (32.26)	-2.896*** (-40.35)	-2.916*** (-44.26)	-2.227*** (-31.53)	-2.268*** (-34.28)
<i>PolicyStringency</i> × <i>SARSCase</i>	0.156* (1.78)		-0.341** (-2.26)		-0.420*** (-2.74)	
<i>PolicyStringency</i> × <i>SARSDeath</i>		0.368** (2.11)		-0.685*** (-2.64)		-0.551* (-1.92)
Country FE	YES	YES	YES	YES	YES	YES
Weekday FE	YES	YES	YES	YES	YES	YES
Observations	20,540	20,540	20,621	20,621	20,621	20,621
Adjusted R ²	0.740	0.742	0.787	0.788	0.667	0.666

Notes. This table presents the results of panel OLS regressions of residential mobility on SARS experience at the country-date level. Mobility data are obtained from Google COVID-19 Community Mobility Reports (<https://www.google.com/covid19/mobility/>). The sample period goes from February 15 to July 27, 2020. For mobility measure in each category, it is computed as the percentage change compared to a baseline. The baseline value is the median value, for the corresponding day of the week, during the 5-week window from January 3 to February 6, 2020. *Stay-at-Home* is the daily percentage change in the duration of time spent at residence compared to the baseline value for that day of the week. *Transit* is the daily percentage change of the number of visits to transit stations compared to the baseline value for that day of the week. *Go to Workplace* is the daily percentage change of the number of visits to places of work compared to the baseline value for that day of the week. See more details on the Google mobility data at: https://www.google.com/covid19/mobility/data_documentation.html?hl=en. *PolicyStringency* is the sum of stringency indexes for all containment measures (i.e., school closure, workplace closure, public event cancelation, restrictions on gatherings, public transport closure, stay at home orders, restrictions on internal movement, and international travel controls) in each country on each day, where the stringency index for each containment measure is obtained directly from the Oxford COVID-19 Government Response Tracker (OxCGRT, <https://github.com/OxCGRT/covid-policy-tracker>). *SARSCase* (*SARSDeath*) is an indicator denoting that the country had SARS cases (deaths). Country fixed effects and weekday fixed effects are included in all columns. Weekday fixed effects denote the fixed effects for each day of the week. All dependent variables are in percentage points (i.e., multiplied by 100). Standard errors are clustered at the country level, and *t*-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

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